ENGINEERING FAILURES AND THEIR LESSONS

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GODFREY'S TABLES

(A 220-PAGE BOOK OF TABLES FOR STRUCTURAL STEEL DESIGNERS, PRICE $2.50)

STEEL DESIGNING

(A 480-PAGE BOOK ON DESIGNING OF STRUCTURAL STEEL WORK, PRICE $2.50)

Published by the Author
EDWARD GODFREY
MONONGAHELA BANK BLDG.
PITTSBURGH, PA.

PRICE $3.00

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INTRODUCTION

A FABLE

Once upon a time it was the universal belief among scientists and all others, every mathematician on earth included, that the ratio between the circumference of a circle and the diameter was three and one-seventh. In those days there arose one Average Man who had an inquisitive turn of mind. In school he had been voted a general nuisance by the masters because of his habit of asking for the ground and reason for certain Established Facts. The usual reply that he received was that Professor Soandso had said so.

Now it happened that one Obscure Man, who had demonstrated that the square on the hypothenuse of a right triangle is equal to the sum of the squares on the other two sides, had died and one Eminent Professor had dug up and published his demonstration, that had been rejected by all of the scientists of Obscure Man's day. Now Obscure Man had met his death by "panning" at the hands of the highbrows of his time, so that no further honors or credit were due to him. By reason of the established position of Eminent Professor the mere announcement of the triangle problem had been sufficient to place the solution in the category of Established Fact.

On the basis of this demonstration Average Man put forth a proof that the ratio between the circumference and the diameter of a circle is 3.14159265 with some more figures to follow, when he had some spare time to work them out. It is a curious fact that this proof was at once published, doubtless because the members of the Publication Committee of a great society were not yet aware that here was an iconoclast, one who tears down, one who would controvert Established Fact.

Up to this time Average Man was considered by most folks to be a mild mannered individual; even little children would come up and eat out of his hand. But he was so ignorant of human psychology that he fondly imagined that his proof would be welcomed, by men who knew, as a contribution to the mathematical knowledge of his day. To his great surprise his proof was savagely attacked by every Eminent Authority of his time. The arguments they used were these:

1. It is contrary to standard practice.
2. It should not go unchallenged.
3. Professors Blank, Blank, Blank, and many others have proven otherwise by numerous tests, that it is not necessary to mention.
4. It controverts Established Fact.
5. It is an insult to the gods, who have given us this beautiful combination of the sacred numbers three and seven.

6. Who is this Average Man?

7. How does he expect to get anywhere by contradicting authorities?

Much publicity was given this paper for by so doing men believed that Average Man could be knocked so dead that he would never attempt to be heard in public again. Average Man carefully answered all of the foregoing arguments. But like the tramp who inadvertently asked for pie the second time at the same back door and was met with the question "Aren't you the same man I gave pie to last week?" Average Man found that he would never be the same man again.

His proof was so clear and simple and incontrovertible, and the arguments against it so fatuous, that sarcasm and antagonism just naturally worked their way into his writings.

Average Man, to the utter astonishment of the Profession, who thought that he had been thoroughly panned and deeply buried under a tremendous weight of authority, continued to write in the support of his demonstration: but the Professors merely remarked, "A rare bit of humor," and gave him no further recognition, neither would they deign to make any reply.

"ESTABLISHED FACT" VERSUS PROGRESS

More than three hundred years ago old Dr. Fracastorius wrote a medical book in Latin. In that book he made the shrewd guess that diseases are produced or carried by, and in turn produce, minute organisms or seeds. His guess, of tremendous and vital importance, remained buried in his Latin volume for three centuries. In the last century Pasteur came along and proved beyond a scintilla of a doubt, by ocular evidence, that the guess of Fracastorius was correct. Pasteur was not a doctor but a chemist; but one would think that when he announced his discovery and advised the doctors to scrub their hands when they went from one case to another, the doctors, of all people on earth, would understand and would give heed to so reasonable a proposition, especially when the microscope demonstrated the truth of his assertion. Instead, the medical profession bitterly fought and persecuted and ridiculed Pasteur because he "controverted established fact."

If Pasteur had been content to bury his findings in a Latin tome or in the journal of a chemical society, they would probably have lain there for another three centuries.

The fact that under pressure on dams exists and destroys the stability of dams was stated in a dignified way before an engineering society, and the statement lay buried in the journal of that society decades before any reference whatever was made to it in any engineering periodical or in any book on dams. And all the while dams were failing by the score from exactly this cause.

In 1904 I began an agitation of the subject of under pressure on dams in an effort to convince engineers that dams must be designed to be stable against

under pressure. I did not hesitate to characterize any other course in the designing of dams as criminal.

For writing in this manner I have been very severely criticised. In 1908 an editorial writer referred to me as a destructive critic. Speaking of the status of the design of dams he said in an editorial in an engineering periodical that under pressure was well understood by engineers and was taught in reputable colleges, though books were silent on the subject. I at once sent a problem to each of his graduating civil engineering students, asking them to work out the width of masonry dam for stability against the forces exerted on it. Not one solution made use of under pressure. I learned from one of his students, now a very prominent engineer, that

(1) He did not think I was far enough advanced to know about under pressure.

(2) The class had designed a very high dam the year before but neglected under pressure.

(3) The Professor had this year revised his lecture notes to include under pressure.

(4) He believed the revision was due to my agitation of the subject.

Professions are not progressive. They will not listen to anything that is different from what the college handed to them with their diploma. The engineering profession is no exception. It is a provision of the constitution of a professional engineering society that no paper will be accepted for publication which controverts established fact; and this safeguard against progress is considered of such benign value that it is repeated in every year book and every pamphlet on the aims of the society which it issues.

A few weeks ago a prominent engineer and professor, in response to my statement that failures are practically all due to a few types of bad design, said in a letter to me that if this is the case I would be doing a great service by bringing up the subject for discussion before the Society. I told him that I had been trying for sixteen years to do that very thing but without any success.

If I listened to advisers, this book would be a bare and bleak recital of engineering principles that should govern in the design of hypothetic structures. An engineer, who publicly characterized the books on reinforced concrete as not worth the paper they are printed on, a specialist in that line, was asked to participate in a discussion of reinforced concrete failures. He declined with the statement that "Reinforced concrete has not failed." By this same token we might say that steel and wood and masonry have not failed, therefore let us ignore the "accidents."

When anything offensive is found in a bee hive, which the bees cannot remove, it is covered up with beeswax. Professional beeswax is very frequently and freely applied, when a structural wreck occurs, to obscure the ugly fact that the failure is due to the design, and the design is very often the

standard, accepted design for which the leaders of the profession are responsible. The beeswax consists in long irrelevant descriptions; intricate mathematical cloaks; results of a search for the block of wood, the shovelful of dirt or shavings, or the bad batch of concrete that brought down a million cubic feet of reinforced concrete building.

Anyone who has given the subject of structural failure any thought or study cannot fail to note that improper design is almost never referred to as the reason for a failure.

If reinforced concrete or any other kind of construction were inherently so uncertain as to its strength that one less salamander would bring down a 200-room hotel, or any of the slips or oversights on the part of the construction gang, already referred to, could be responsible for a great wreck, it is time that such construction be totally avoided. Absolute perfection of workmanship is not to be expected. But fortunately reinforced concrete is not of this nature. When a design is proper, poor workmanship or poor materials may be cause for a local failure but not for a perfectly general and sudden collapse.

Editors and publication committees give large space to matter that bristles with mathematics or abounds in impossible theories that seek to place the blame of wrecks on some mysterious thing that no man could possibly anticipate and that no man ever will refer to again. A large amount of engineering literature is of no value whatever and medals have been awarded to authors of some of this.

Many times I have been refused space to bring to engineers' attention clear demonstrations of the cause of structural failures that are just as definitely proven as any proposition in Geometry ever was. I have in mind a case where forces definitely calculated to be 15,000 lbs. each were unprovided for in the design. The structure failed. Not one word was said concerning these forces in accounts of the failure. I was estopped from warning engineers against this kind of designing, though in my letters and articles I cited an exactly similar case where incipient failure was arrested and prevented.

I have in mind another case where forces amounting to hundreds of millions of pounds were ignored in the design. I was informed that I was too far away from the scene of this wreck and slaying of a half a thousand people to pass an opinion on it.

The manuscript for this book was ready for publication thirteen years ago. Every year since that time has added its meed of wrecks confirming most emphatically everything written and ready for that earlier book. Some chapters have been left just as they were written for publication in 1911: the additional matter has been added as a second part.

The things set forth in this book are not matters of opinion; they are not private views; they are definitely proven facts.

The door to argument is closed. I have held that door open for nearly

two decades, have invited, challenged, urged men to put up some kind of argument in defense of the standards of design that I have criticised, but the arguments are not forthcoming. The following facts have contributed to the closing of that door.

(a) In 1915 at the American Concrete Institute Convention at Chicago a long discussion of my paper criticising standards of reinforced concrete design took place. My paper was condemned and criticised by everyone who took part. I was urged to write a complete closure. This I did, after waiting one year for typewritten copies of the remarks. I urged publication of this discussion. It has never been published. I do not hesitate to say that if I had set up the arguments against the paper which were contributed, I should have wanted to see them suppressed.

(b) In 1916 the Joint Committee on Concrete and Reinforced Concrete, after ordering me by resolution to go to Atlantic City to their final meeting and defend myself for the offense of criticising their report, at first refused to give me any time for a hearing, and then listened with a sneer to my 20,000-word defense and went ahead with the report approving types of design that members of the Committee had refused to discuss publicly with me, types that have contributed millions of cubic feet of building wrecks to the long and ever growing list of failures.

The maker of the motion demanding my appearance at Atlantic City to defend myself said, "We want evidence, evidence that would stand in a court of law. We are tired of his personal views." My dissenting note in the 1916 Report is the price of my remaining on the Committee.

(c) In 1917 the Publication Committee of the American Society of Civil Engineers refused to publish a word of mine discussing the failure of the Stony Creek Dam, which was due to the standard method of design—neglecting under pressure. The same year they refused to publish a word of mine discussing the Joint Committee Report of which Committee I was a member.

(d) In 1920 the Publication Committee of the Western Society of Engineers refused to publish a paper of mine on shear reinforcement in beams on the grounds that they did not agree with me and feared the effect on minds not set as theirs were. (The paper had previously been tentatively accepted.) It is not the rejection of this paper so much as the reason for that rejection that helps to seal the door to argument.

(e) In 1922 the Secretary of the Western Society of Engineers, solicited a contribution to the discussion of a paper on the Knickerbocker Theater and the Salina Masonic Temple failures. He thanked me for the same and said it was just what they wanted and would be published. The Publication Committee acting in 1922 vetoed this, and after waiting many months I was informed that my contribution would not be published, because so many theories were advanced it was impossible to prove who is right. Here, again, it is the reason for the rejection that is significant.

(f) The Joint Committee which reported in 1920 at first ignored a letter of mine in which I asked for the privilege of reviewing and criticising what I knew would be the provisions in the matter of design of their forthcoming Report. (My anticipations were entirely correct.) I had asked to be privileged to make these criticisms and then to have any or all of their members reply and then to have the opportunity to reply to their arguments. When I saw that my proposal was ignored I began this criticism in *Concrete* of Detroit. The Committee then asked me to appear before them in New York. This I did for a day's session. The only reply to my arguments was heckling and flat denial of statements of mine that I subsequently proved to be correct.

(g) In February, 1923, a paper of mine was read in London before the Institution of Structural Engineers. It was on the subject of Shear Reinforcement in Reinforced Concrete Beams. This paper criticised all of the standard works and all of the standard specifications on reinforced concrete. Copies of the paper and the London discussion were sent to twenty of the most prominent reinforced concrete engineers and authorities in America with the invitation to discuss it and defend, before the London audience, their own works and utterances. The majority ignored the paper and the letter. Only one man contributed to the discussion and he did not utter a word in defence of his utterances criticised in the paper.

These seven incidents, to say nothing of the unnumbered occasions when I have publicly condemned and criticised the standards of design that in this book are held to be responsible for the failures of record, and the silence with which these criticisms of standard design are met, all point to one fact, namely, that there is no argument for these condemned standards that will stand the light of day.

If the leaders of the Engineering Profession can give no better excuse for the awful harvest of wrecks of the last few decades than that which I have elicited, it is time that they give up the conduct of affairs to a new generation of men, who have not committed themselves and have, therefore, nothing to retract, and who recognize simple and incontrovertible principles of mechanics.

I quote here "Exhibit X."

This is a letter of mine to the Editor of the *Engineering Record,* published in the issue of Jan. 25, 1913, p. 112.

"Dear Sir: Your editorial on 'Safety in Concrete Construction,' in the issue of Dec. 28, is a commendable one and should be carefully read by all reinforced concrete designers. One sentence that is especially notable, if not startling, is this: 'Few engineers really understand the internal stresses in monolithic structures, and lamentably few designs when considered with this point in view are found adequate.' It is so easy to blame a failure on the general cussedness of cement or the weather. One of last summer's failures was blamed on 'cold weather.' The weather was not all that the sea bather

could wish, but it is asking too much of the easy public to try to put one like this over on it.

"Another notable part of your editorial concerns the engineer who was compelled to drop reinforced concrete designing because his conscience would not permit him to do it in the standard, job-getting, wreck-breeding method. The engineer is not alone in having his plans turned down because they are safe, and the Cincinnati owner is not the only owner who has taken over the role of designer and judge of safety. Fortunately, however, sometimes building departments intervene between the owner and the coroner.

"You say: 'Poor materials are shown up in a comparatively short time, but inadequacy of design is more likely to be responsible for defects which do not develop until three, four or more years after construction.'

"You say again: 'More stringent standards have been proposed.'

"It is these four features of your editorial that I should like to discuss and enlarge upon.

"The most sinister fact regarding the knowledge of engineers and others engaged in this work is that they not only do not know, but they refuse to learn, or even to discuss, the stresses in concrete. A specialist in this line refused to attend a meeting or to discuss a paper on reinforced concrete failures because he did not agree with the author of the paper. This is a sample of the kind of minds that are being trained on these problems. At this meeting were nearly 200 engineers and architects. The author of the paper put forth a large number of arguments and criticisms against standard reinforced concrete design. Not a single one of these criticisms was refuted. No arguments whatever were forthcoming to sustain these standards that have characterized every reinforced concrete wreck.

"Developed defects are matters that scarcely ever come to public notice, for obvious reasons. When a patient is operated upon the operation is recorded in the medical journals as successful if he lives to regain consciousness. When a building is constructed it is recorded as an engineering success if it stands up until the contractor gets his money. Of course, the subsequent death of a patient or subsequent defects of a structure, if they occur, are merely incidents and have no place in professional histories. They are due to overloading of the stomach or the floor, as the case may be. A few days ago a frightful wreck took place on one of our great railroads because twenty-two years ago a structural engineer allowed a structural blunder in steel, and because during those twenty-two years no structural engineer had discovered and corrected this easily detected blunder. A large amount of sand was thrown in the public eye by means of reams of irrelevant testimony as to the length and weight of the train and other useless stuff. We are told that no structural engineer was asked to testify—this is a matter that could have been fully explained by a structural engineer in three minutes. The end post of a girder (the end stiffeners) was not placed over the post sup-

porting that girder. The simplest kind of an engineering principle was violated; result, a wreck twenty-two years later.

"The commercial flat slab (the one that can meet competition) depends to a very large extent for its strength on tension in the concrete. How long will this tensile strength hold out under the shock of rolling loads, as for bridges and railroad viaducts? No analysis can show that a flat slab, as usually designed, would stand up (under safe tensile stress in steel alone) if a row of bays from wall to wall be loaded; and no tests have been made, so far as I can learn, of a building loaded in this way. Also no analysis can show safety of an outer row of bays supported on posts only near the edge of slab. No tests have been made on a row of such bays. In fact, I know of no test on a single outside bay supported on posts and not on a deep girder or a wall.

"There are some inexcusable standards in steel designing; in reinforced concrete their name is legion, and the shame of it is that most of them are sanctioned by practically all of the books on the subject. These authors refuse to discuss these faults even when they are publicly pointed out in their own writings.

"*Rodded columns*—columns of plain concrete with a few upright rods in them—are strong (sometimes) in a testing machine, where the load is exactly central and when no expansion of a monolith can spall off the corners. Find them in the Cincinnati wreck or in a large number of other great wrecks. They are usually to be found located in the basement in broken chunks.

"'*Shear' rods*—vertical, inclined, attached, floating—why did they not take the end shear in the endless list of wrecks where girders and beams broke off square at the supports? There is not in all engineering literature an analysis that shows how these so-called shear rods, that characterize nearly all reinforced concrete design, can take the shear of a girder or any other stress of any kind commensurate with their size and the expense of their installation. I have repeatedly tried to elicit some kind of analysis from the innumerable users and advocates of these foolish rods.

"*Bunching of rods in the bottom of a beam.*. Look at the Cincinnati wreck! Almost a solid layer of rods. And yet a standard allows this: ¾ in. clear between rods horizontally and ½ in. clear between them vertically. No analysis can possibly show that concrete can grip this steel or carry the shear incident to its stress. The standard just referred to is in one of the most recently proposed building codes. It does not look as if more stringent standards are being proposed by the 'interests' responsible for building codes.

"Editorials such as this one, often repeated, ought to go a long ways toward waking up the engineering conscience. Yours truly,

"PITTSBURGH. EDWARD GODFREY."

I will be criticised for quoting from my own writings and not re-writing

these things in the way that books are usually made. This book lays no claim to being usual. It is an indictment against men who are responsible for standards of design that have bred the countless wrecks of history, men who refuse to listen to or to answer arguments against those standards of design, but who go on and reiterate the standards year after year. The quotations and citations in this book are exhibits in that indictment. They are the "evidence." They prove that for two decades I have been endeavoring to point the way to safe design and steer men away from the rocks of violation of principles of stability.

This book contains an account of a large number of structural wrecks, all of them of types of design that within the last twenty years I have publicly condemned. A large number of the wrecks occurred in structures designed and built after the type of design was publicly condemned. Standard works do not condemn these types of design nor give warning as to their unsafe character. Practically no writers in engineering periodicals condemn these types of design nor give warning as to their unsafe character.

The "standard" alibi for all wrecks is poor materials or poor workmanship.

No wrecks of any consequence or of any considerable number (barring such accidents as explosions or collisions) are on record that cannot be explained by these condemned types of design.

No engineer in America has been more freely and frequently damned than the author of this book.

The wrecks of the past few decades have been so numerous that they cry to heaven for reform, and the voice of a reformer should be a most welcome sound to anyone who has at heart the preservation of property and human life. The principles of safe design laid down in this book are not generally accepted by engineers, but they will be so accepted. I believe I have vision enough to see that every principle of good design set forth here will eventually be universally accepted by the profession that now trembles lest someone "controvert established fact." These are not the words of a braggart. It is given to some men to see things to which other men's eyes are blind.

But this does not lessen the sting of the rebuke that is due the men who sit in high places, and sit tight, and refuse to discuss or consider the pleadings of one who would reform a system of design that has strewn recent decades with almost uncounted and almost wholly avoidable wrecks. It does not excuse the men who incorporate into the constitution of their society, and act upon, such provisions as refusal to publish anything that controverts alleged established fact, this clause that dams progress and damns safe design.

I have only sympathy for the engineer who inadvertently errs in the making of a design, who follows standard but erroneous practice in his design, and who is not informed as to the erroneous character of that standard, and whose structure fails by reason of the design.

To err is human: to persist in known errors is devilish.

CHAPTER I

DAMS

Part I. Written in 1911.

Among engineering failures probably the most common are those of dams. Unfortunately too, they have been the most disastrous in the way of loss of life and property. But saddest of all, this loss is largely a preventable one, if men would open their minds to logic or would give some rational study to a matter that so vitally concerns the public, as well as their clients who are furnishing the means to build.

The reason why so many dams have failed is simple in the extreme. It can be understood by men who have no engineering education or training. It has been missed by men who have made a specialty of the design and construction of dams. Simply stated it is failure to consider under pressure, that is, pressure in the horizontal joints and under the dam. It is a universally known principle in hydraulics, among persons of only common school education, that water exerts pressure equally in all directions at any given point, that that pressure is dependent upon the head and independent of the volume of water that may lie above the given point. The water may rise from a barrel into a small tube high in the air, as in the old time test in the old time "Philosophy," and the pressure of a small quantity of water may burst the barrel. It may rise from a small tube into a large reservoir: the unit pressure will be the same for the same head. The ocean would exert the same lateral pressure against a sea wall as a thin sheet of water confined against the same wall. The water may soak through a sponge, or a porous masonry joint, or porous concrete, or permeable earth; the result is the same: wherever the water is confined, it will reach a pressure corresponding to its head, just as though it were in an open tube.

It is only a few years ago that concrete experts were proclaiming that it is impossible to make concrete impermeable. Cases were cited where water flowed freely through 30 feet of solid concrete, as it may do in the dry rammed concrete so common in earlier days. A large number of dams are made of this kind of concrete.

UNDER PRESSURE IGNORED

These facts are absolutely incontrovertible. Their recital here would seem puerile, if it were not for the further fact that they have been virtually denied or evaded time and again, and, with one recent exception, are totally ignored in books on dams. When dams have failed in recent years, and

under pressure is suspected, subterranean fissures are invented to carry the water under the dam; whereas in building the dam, in all probability, the capacities of some good-sized steam pumps were taxed to keep the foundation unwatered, because of water that just naturally soaked into the pit. In 1900 the dam at Austin, Texas, failed. In 1908 an engineering report discovered that the failure was due to under pressure and that the water reached the bottom of the dam through fissures: as though it would be impossible for water to reach a given point beneath the surface of the ground without a bore-hole or a cavity of some kind for it to traverse.

THEORIES TO EXPLAIN AUSTIN DAM FAILURE

At the time of the failure of the Austin dam many theories were brought forth to account for the failure. One of these was "vacuum on the down-stream face." Under the stream of water as it falls over the crest of a dam, where the stream contracts, there is a diminution of the air pressure. This "vacuum" might, on part of the face of the dam, under unusual conditions, raise water a few inches. It could scarcely amount to as much as a good wind against the dam. It is hardly possible that it can ever compare with the loss of head in the water from the main body to a point just over the crest. In a correct design the full head of the main body of water should be considered, whereas in fact the static head will be measured by the height of water vertically over the crest. By taking the full head of the main body of water the impact of the moving water, as well as any slight reduction of pressure on the down-stream face, will be provided for.

BLIND SEARCH—RUBBER MODELS—SHEAR

Another line of blind search for the elusive cause of failure in dams has led investigators to make elaborate calculations of shearing stress in dams and to test rubber models in attempts to prove the hypotheses. Rubber is utterly foreign to the materials with which dams must be built, and its properties are hopelessly variant from the properties of those materials, so that such investigations are worth little except as a scientific exercise. Shear combined with tension, as will be shown, may have much to do with the failure of dams of incorrect cross section, but this is a matter that can almost be detected by the eye and needs no fine-spun theory.

In 1895 Mr. John D. Van Buren, in a paper read before the American Society of Civil Engineers, suggested that the pressure of water under a dam is worthy of consideration in making the design. A hint is given in Trautwine of need of such consideration, where it is said that if the pressure on the masonry reduces to zero, water may work into the joint and act as a wedge. (This is under retaining walls and not dams.) Some foreign writers have given voice to the same fear. No book on dams, published prior to 1910, makes any mention of this under pressure. In

Engineering Record, Dec. 3, 1904, without knowledge of Mr. Van Buren's suggestion, I pointed out this force as being the probable cause of the failure of a reservoir wall. In the same periodical in subsequent issues, and elsewhere, I have publicly called attention to the necessity of considering the upward pressure in all designs of dams.

FALSE IDEAS ABOUT UNDER PRESSURE

The idea that water can enter masonry joints only when the pressure on these joints is nil is preposterous and dangerous. Water could flow with ease through a masonry joint which is under very heavy pressure. In fact pressure would have no sealing tendency whatever in a masonry wall. If masonry were compressible and by being compressed were rendered less permeable, the pressure would be a factor in preventing admission of water; but the mortar in a masonry joint may sustain heavy pressure and yet be porous or even honey-combed with cavities.

When masonry is actually submerged, no question is ever raised as to the fact that it loses about 62.5 pounds per cubic foot of its effective weight. The only difference between a block of masonry having vertical sides, submerged in water, and the same block acting as a dam with the water on one side only, and exerting its pressure beneath, is the absence of the horizontal pressure on the other sides. This horizontal pressure can have no effect on the weight of the block. The loss of weight due to the water in each case is the same.

It is practically impossible to seal up the entire bed of a stream or other body of water, and it is impossible to prevent water from soaking into the soil below a large body of water. That water will seek an outlet, or, if confined, will attain a pressure corresponding to a head gaged by the level of the surface of the stream.

DANGER IN STOPPING LEAKS

The stopping up of leaks in a dam can most readily be accomplished by plastering up the down-stream face. This only serves to confine the water in the joints and intensify its pressure, thus lessening the stability of the dam.

TESTS SHOW PRESSURE AFTER PENETRATING MORTAR

It has been proven by trial that water will penetrate 18 inches of good sound cement mortar and register practically the full pressure on the other side of the same. Numerous tests have been made on the pressure of water that reaches the base of dams, and high pressures have been recorded, pressures nearly as great as that corresponding to the head of water in the dam. The very fact that the pressures are large and not equal to the full head is proof that the water recording such pressures does not enter through fissures, or a full head would be recorded.

Various methods have been suggested to overcome or prevent the under pressure of water on a dam. The method which receives the least consideration is the simple one of making the dam of sufficient stability to stand against the pressure. This is simply a matter of adding masonry so that the dam will be heavy enough to resist all possible horizontal and vertical forces that tend to overturn it.

FUTILITY OF UNDERDRAINAGE

Underdrainage has been suggested and even practiced. To be effectual this would require that the dam be fairly honey-combed with ditches underneath. To be sure and permanent the outlet of this drainage system must be forever prevented from silting or freezing up. The whole idea of underdraining a dam is absurd on the face of it. Dams are built to retain water. Many of them are designed with the express idea of retaining every gallon of water possible. Water that leaks off is wasted and is a continual loss. A leaky dam that is underdrained could scarcely have the leaks stopped without menacing the underdrainage. If the underdrainage silts up, which it may readily do, with unlimited quantities of mud and water to draw from, the drainage system becomes useless; and if the stability of the dam depends upon this underdrainage, what of that stability? Or in a stream of small flow, what if the outlet of the underdrainage system should freeze up? The water will back up into the drain, attain its full head, and, in at least one exploited style of dam, would almost surely carry the dam away. It is a crime to build important structures whose safety depends upon such slender threads and whose failure could be brought about by such trivial and natural causes.

CUT-OFF WALLS INEFFECTIVE

Another method by which attempt is made to obviate the under pressure is to build a wall along the up-stream edge several feet below the general level of the base of the dam. The obvious intent is to intercept the flow of the water. Some add another wall at the down-stream edge, to confine the water that gets past the first—a confession that the first will not serve its purpose. Water does not have to flow through a free passage way or an uninterrupted stream in order to exert pressure. It may work its way through a most circuitous route and still exert a heavy pressure at the end of that route. It may readily pass down below one or more intercepting walls and exert a pressure on a horizontal surface beyond the same. The dam shown in Fig. 1 is the Pittsfield dam, which failed Jan. 7, 1909. It was completely underwashed, as indicated in that figure. (See *Engineering News,* Apr. 1, 1909.) The up-stream wall of this dam was 8 feet deep below the under side of the slab and the down-stream wall was 5 feet deep. It is a serious fault in this dam that it has a horizontal reinforced concrete slab to get the full upward pressure of any water that gets past the up-stream wall and exerts its stability-de-

stroying pressure under the structure. It is true that there are weep holes in this slab—another confession that water will find its way under the slab. If the material of this slab had been used in building a deep up-stream wall, not with the intent of preventing all water from passing, but with the purpose of preventing the carrying of sand away from beneath the dam, the design would have been enormously improved. The buttresses of this and other like dams should have footings only large enough to give a safe pressure on the soil. A slab whose manifest purpose is to prevent the escape of water, provided with weep holes to allow that water to escape (if they do not silt or freeze up or fill with clay), is another absurdity in the way of a drainage system.

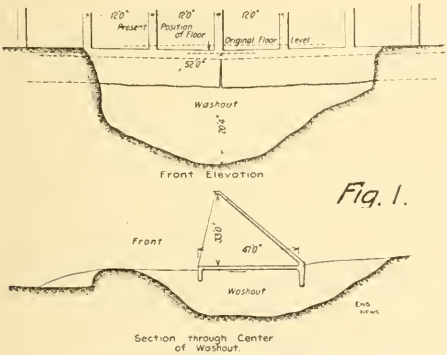


Fig. 1.

FRONT ELEVATION AND SECTION THROUGH THE WASHOUT UNDER THE PITTS FIELD DAM.

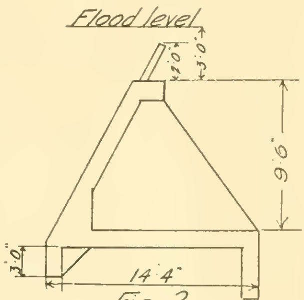
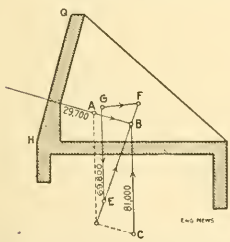
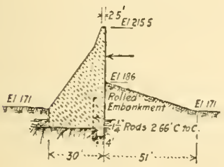


Fig. 2.



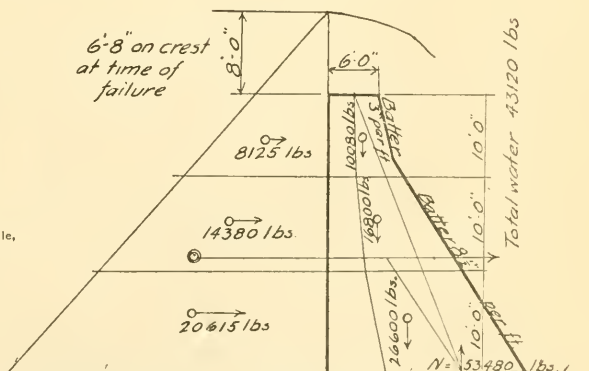
Approximate Stress Diagram of Dam at Dansville, N. Y., Under Water Level at Top of Dam.

Fig. 3.



Section A-B.

Fig. 4.



Section of Lower Tallassee Dam-Tallassee, Ala. This is a stone masonry dam.

Fig. 5.

The dam shown in section in Fig. 2 failed by water getting under it. (See *Engineering News*, Jan. 13, 1910.) Here the deeper intercepting wall is

at the down-stream edge, showing the designer's desire to confine the water which the upper wall "prevented" from passing. The dam shown in Fig. 1 failed by under-washing, but the structure held up, sagging only over the cavity. The dam shown in Fig. 2 failed by under pressure and was under-washed after a portion of the dam pushed out. Both are illustrations of the futility of attempts to prevent water from getting under a dam. It is one thing to stop the flow of sand by an intercepting wall reaching below the sand bed; it is quite another thing to stop the percolation of water through soil and to inhibit its inevitable tendency to accumulate a pressure corresponding to its head.

Fig. 3 is a sketch given in connection with a letter of mine published in *Engineering News*, Jan. 27, 1910. The forces shown are approximately those acting on a bay of the dam shown in Fig. 2. The following is quoted from my letter:

DESIGN SPELLS FAILURE

"There is nothing at all surprising about this failure. To me it would have been a surprising thing if it had stood the pressure of water behind it for any considerable length of time. I have shown in a sketch herewith the general shape of the dam and the forces acting on a bay. The net resultant of these forces is G F. This force does not strike the middle third of the base. It does not strike the base at all. It does not even strike the ground. It has a rising inflection, so to speak, and would tend to give the dam a down-stream and upward course. Pressure on the flash boards was ignored in figuring these stresses, but the assumption of such additional pressure would show diminished stability.

"The forces acting on the dam are: A B, the water pressure on the sloping face Q H; B C, the water pressure beneath the slab; and G E, the weight of the dam.

"It may be contended that the force B C, due to pressure under the slab, was not intended by the designer to act. But designers' good intentions are not always respected by water and materials of construction, and designers' assurances that structures are acting as they were intended to act are not always convincing.

"The force B C, 81,000 lbs. in my sketch, is due to water pressure under the slab. Your article states that witnesses say that water was squirting up out of weep holes in this slab. But assurance is given by the engineer that the gravel below the dam was not wet. Both statements may be correct, as the gravel below the dam may have been porous enough to carry off this leakage in a sub-stream. Porosity in the gravel would increase pressure under the slab.

"There are some very pertinent questions that could be asked regarding this dam and dams of like construction.

"Why was this slab used in the design? The pressure on the soil is not so great as to need all of this spread. If it was used to prevent leakage of water that passed the toe wall, would this not mean upward pressure on this slab? Would not pressure of water beneath this slab greatly impair its stability, especially if it was not designed with stability enough to resist such pressure?

"Why was the heel wall made relatively so much deeper than the toe wall? Is this not to confine the water under the slab? Would it not have been better to add this entire wall to the toe wall and omit entirely the heel wall? The heel wall was not made a girder, though the slab was reinforced as a load suspended upward from it. The slab could have been reinforced from buttress to buttress, and this would have given the same soil pressure, if soil pressure were the thing the slab was designed to give.

DOUBTFUL EFFECTIVENESS OF WEEP HOLES

"Why were weep holes made in the slab? Does this not indicate that it was anticipated that water would get under the slab? Weep holes would relieve the pressure in the immediate vicinity of the several holes. They would prevent the water, in spots, from accumulating and reaching the pressure due to its head, but unless the water flows freely, it will exert a pressure. Nothing but a sieve-like construction would be free from pressure, and such construction would defeat the manifest purpose of the slab.

"In just the proportion that the base slab is useful in retaining the flow of water it is a menace to the stability of the dam by reason of the pressure of the water retained.

"This type of dam condemns itself, when made in proportions approximating this example; because on the face of it, elaborate preparations are made to prevent the escape of water that works its way beneath the dam, and a little graphic computation will demonstrate that the dam is rendered unstable by the pressure of this water in seeking an outlet."

"GUARDING AGAINST" UNDER PRESSURE

The dam shown in section in Fig. 4 failed in Jan., 1910. (See *Engineering News,* March 17, 1910.) Here is what is said of it in *Engineering News:* "The most important cause, however, was due to the water getting under the dam. This condition was not anticipated when building the dam, and all precautions which seemed to be necessary at the time were taken to guard against it." This is the dam at Austin, Pa. As I write this the country has just been made aware of an awful disaster due to a second failure of this dam, which occurred Sept. 30, 1911. I wonder if this failure too will be glossed over and men will continue to ignore the simple lessons of these awful disasters. It is astounding that failure after failure can occur with sickening regularity, and yet men who claim to be experts, professional men, specialists, will remain totally blind to simple principles of mechanics that

can be absolutely demonstrated and can be made perfectly clear to any intelligent high school boy.

As I have shown in my book "Concrete" (p. 388), the width of a dam at the base should be .85 times the height, in a cross section such as shown in Fig. 4, in order to be stable against the upward pressure with water reaching to the crest only. In Fig. 4 that width is only .6 of the height.

Fig. 5 shows the cross section of a dam that failed. This is described in *Engineering News,* Feb. 13, 1902. The figure shows a graphical study that demonstrates (?) the stability of the dam. Under pressure is ignored. The base of this dam is .74 of its height, and yet it is supposed to be stable against a surcharge of 8 ft. of water.

Fig. 6 is another dam which failed. (See *Engineering News,* Jan. 9, 1902.) This one is .71 of the height in width of base.

Fig. 7 is the section of still another dam which failed. (See *Engineering Record,* May 9, 1908.) The account of this failure is given in *Engineering Record,* June 27, 1908. Fig. 8 is from *Engineering Record,* July 11, 1908. This is a sketch given in connection with a letter of mine. The following is quoted from that letter:

SUBTERRANEAN SEAMS OPEN UP (?) UNDER PRESSURE

"The account states: 'The local impression is that some seams in the sandstone of the river bed were opened, by the high head of the flood, enough to allow an upward pressure to come upon the base of the concrete, and thus enable the impounded water to slide the structure down stream. Accidents of this nature are unusual.'

UNDER PRESSURE NOT MENTIONED IN BOOKS

"Why should accidents of this nature be put in the unusual class? Is it not a usual thing for water to percolate through dams and reservoirs? What can be done besides wasteful underdrainage to stop it? Watertightness under the most favorable conditions is extremely difficult of attainment. Why, then, are not dams designed with a view of maintaining their stability in the event of their not being perfectly watertight? Why do books on dams, some of them of very pretentious proportions, omit altogether even mention of the possibility of the water working under a dam and supplying a lifting pressure that not only may lift half or more of the weight of the dam, but also acts to lubricate it to allow the blocks to slide out? Instead of long discussion of the "theoretically correct" cross section of a dam, books on the subject would do well to say something of a practical nature on how to make a dam that will dam.

"The dam above referred to is described in the *Engineering Record* of May 9, 1908, and the cross section is given. I have traced the same, leaving out the curves to simplify the calculations, and showing dotted the projection

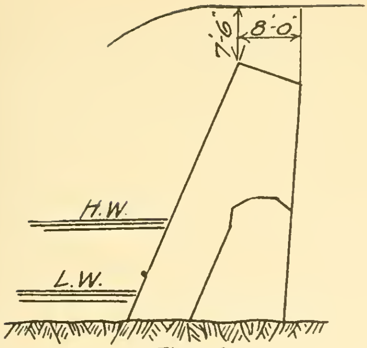


Fig. 6

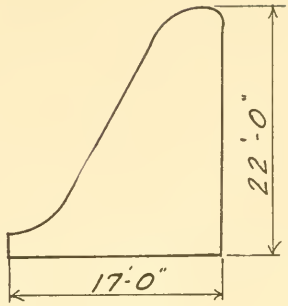


Fig. 7.

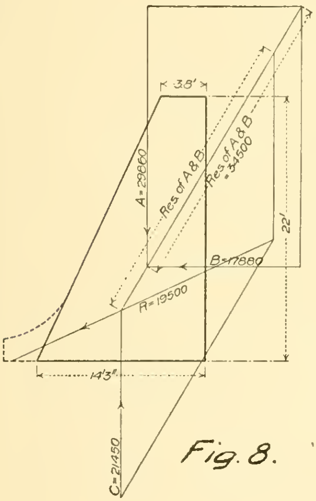


Fig. 8.

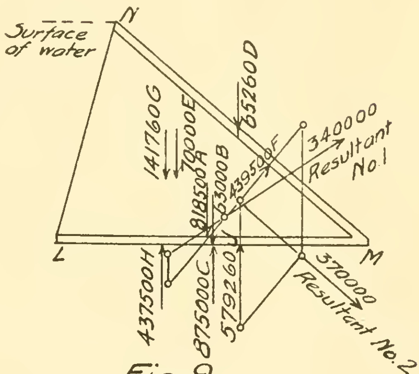


Fig. 9.

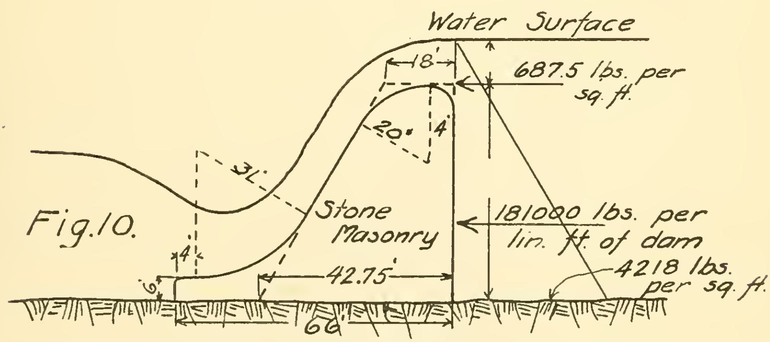


Fig. 10.

Cross-section of Austin Dam with assumed Condition of Flow at time of Failure. (Cross-sectional area nearly 2200 sq. ft. Weight of dam per lin. ft. 308000 to 330000 lbs.

at the toe, as being of no importance structurally, because it is too thin to take the shear, if the dam should rise on the toe. Force A is the weight of a foot of the dam; force B is the horizontal pressure, assuming the water to overtop the dam two feet; force C is the upward pressure of the water under the dam, with a head of 24 feet. Force R is the resultant of A, B, and C. It is seen that this resultant passes clear outside of the base. If the water overtopped the dam by a less amount, of course the resultant would fall closer to or within the base; but a crest of this amount would not be unusual in a flood. The first impulse of this dam would be to rise on the upstream side. This would break it up and allow the lubricated blocks to slide out.

"It is of little importance that the dam did not leak water from the downstream side. The sealing may have been entirely due to tight joints at the toe, where concreting and plastering could best be done. In fact, if the latter were the case, failure to leak would be a dangerous condition, as the water, having no escape, would have its full hydraulic head."

Following the above letter there appeared in the *Engineering Record* a note and several communications and an editorial which aimed to show that these matters are not altogether foreign to engineering knowledge and engineering literature. The author replied to these in the issue of Aug. 8, 1908, excepting the editorial, which appeared in the same issue. The incontrovertible facts seem to be that, with the exception of Mr. Van Buren, who suggested in 1895 before the American Society of Civil Engineers, the need of considering the upward pressure of water in all cases, the universal obsession of engineers in 1908 was that this pressure need never be considered except in the case of fissures or tension on masonry joints. It takes a long time, and sometimes a large number of wrecks before an easily demonstrated fact soaks into the minds of a body of professional men who have not previously accepted that fact.

PROPOSED HOLLOW DAM

In *Engineering-Contracting,* Feb. 1, 1911, a new style of dam was proposed. This is a hollow dam consisting of a horizontal slab and an inclined slab meeting in V-shape with counterforts between, the water being on the side of the opening of the V. In the issue of March 8, 1911, of the same periodical I pointed out the danger of designing dams of this character, using the cross-section proposed as an example. The sketch accompanying that letter is shown in Fig. 9. The following is quoted from my letter:

"In the sketch herewith is shown the cross section of this dam, somewhat simplified in outlines. The dam is 100 feet high and has a base of 140 feet. The slabs are assumed uniformly 3 ft. thick and the counterforts 1 ft. thick, spaced 13 ft. The forces that act on one foot of the dam are shown. A is the weight of the water on the upper side of the slab L M. B is the weight of the slab L M. C is the upward pressure of water under the slab L M,

with a head of 100 ft. D is the weight of the slab M N. E is one-thirteenth of the weight of a triangular counterfort. F is the normal water pressure on the under side of the slab M N. G is the resultant of A, B, C, D, and E. The load of 340,000 lbs., marked Resultant No. 1, is the resultant of G and F, or the total resultant on the dam for the condition assumed, which will be explained presently. It is thus seen that this dam would have a very substantial inducement to float away.

"The reason for taking the force C into consideration may be found in 'Hydro-Electric Plants,' by Beardsley, page 247, where I read the following: 'Mr. J. B. Francis held that solid concrete deposited on bed rock would be lifted or floated, and to prove this placed a pipe provided with a pressure gage in the concrete of a dam and found that the gage registered the full pressure.' Taking this principle and applying the pressure gage along the line L M we should expect to see it register the pressure of 100 ft. head of water. Then Resultant No. 1 shows the direction that the dam would take when water reached the height N and had time to seep into the foundation.

"Or suppose that while the water seeps in at L it is escaping at M and that the pressure diminishes to zero at M. This triangle of upward forces would have its resultant in H. The resultant of all vertical forces is then J, and Resultant No. 2 shows what this combined with F amounts to. This strikes outside of the middle third of the base and would mean tension at L, which in turn means freer access of water under the base and a tendency to reach the condition of Resultant No. 1, at least so long as the dam would remain in place.

"It is seen by the above that the dam would be quite unsafe, unless it could be perfectly sealed against the admission of water beneath it. This is practically impossible."

The designer of this dam replied to the above letter. His chief contentions were: (1) His intention would be to under-drain the dam. (2) Water would be intercepted by concrete-filled trenches or piling. (3) Model tests showed relatively high stability. I replied to his letter in *Engineering-Contracting*, May 3, 1911, from which the following is quoted:

"It would be a crime to build a dam that may lose its entire stability by the mere silting up of an inaccessible drainage trench or the freezing up of its outlet. Dams are built to retain water. Some of them are made with the express idea of conserving every gallon of water possible. In a stream of small flow it would be quite possible for the outlet of a drainage to freeze up. Leaks in dams are common. It is next to impossible to stop up completely all leaks. The only way possible in many cases (of a dam in use) would be to seal up the outlet on the down-stream side. This would only serve to intensify the pressure of the confined water. The best way to make a dam entirely safe is to make it with mass enough to resist any possible horizontal

or vertical pressure that the water may exert against it. It is entirely possible to build a gravity dam with stability enough to resist such pressure.

"It is easy to make models and seal them against a few inches of water with a rubber sheet. It is quite a different proposition to seal up the entire bed of a stream and the surrounding soil against admission of water working 24 hours a day 365 days in the year to penetrate the obstruction."

SHEET PILING TO PREVENT UNDER PRESSURE—THE HAUSER LAKE FAILURE

One of the supposed means of preventing under pressure is to drive sheet piling along the up-stream edge of the dam. This is just about as inefficient as the intercepting wall or concrete-filled trench. It is almost impossible to make a water-tight joint between sheet piling and a slab of concrete. In any event the water may pass around the end of the row of sheet piles and work its way under the dam just as surely as in the other case. A great steel dam was built at Hauser Lake, Montana. It had sheet piling driven along the up-stream edge to keep water out from beneath the meagre concrete "anchorage" for the steel sheets. Water found its way through. The dam failed with great loss.

WHY SOME DAMS DO NOT OVERTURN

A feature of the wrecks of some dams that makes it appear that they do not fail by reason of the under pressure is that they do not overturn but great blocks slide out horizontally. This can be easily accounted for. It is the under pressure that starts the failure. The block is lifted and lubricated by the water beneath it. When this is done, the force of that water is expended. It would take time for it to gather more force. Water can enter but slowly into a narrow joint. In the meantime the horizontal force of the water against the up-stream face is exerted continuously and with undiminished pressure. This carries the loosened block along in a horizontal direction, giving rise to the popular notion that the blocks have simply slid on their foundation and to the theory that the coefficient of friction of the dam on its foundation is very small. (See *Engineering Record,* Oct. 21, 1911, p. 492.)

The obvious lesson of all this is that the most reliable form of dam to build is a gravity dam, that is, a solid concrete dam having mass enough to remain in equilibrium against the full horizontal pressure of the water on the up-stream face and the full vertical pressure of the water on its under surface. As stated in my book "Concrete," it is reasonable to assume that the under pressure diminishes gradually from the full head at the up-stream edge of the base to zero at its outlet, the down-stream edge. In a gravity dam, designed to resist both the uplift and the lateral force, where the water is just

level with the top, the overturning effect of the under pressure on the base is 72 per cent of that of the horizontal pressure of the water on the up-stream face. It is astounding that a force of this tremendous magnitude and importance should have been totally ignored for so many years by men who have made the subject a study and have seen dam after dam wrecked by the exhibition of this force.

DECEPTIVE WIDTH OF BASE OF DAM—THE AUSTIN, TEXAS, FAILURE

Fig. 10 shows the Austin, Texas, dam which failed in 1900. The forces given are those of a "standard" treatment, which shows the omission of the under pressure. This sketch is given here to show another fault in the design of this dam. This concerns the thin toe of the dam. The apparent width of the base of this dam is 66 ft.: the actual width, as a basis for estimating its stability, is about 43 ft. If the weight of this dam were turned up on the thin toe to the left, this toe would shear off. This is a common and standard cross section, and it is common to allow the full width of the base in estimating the stability. This is wrong and misleading, because it gives rise to a false sense of security. This base cannot be made use of as an element of the stability of the dam, because that fancied stability is incompatible with integrity of the dam.

SAND-FILLED CONCRETE SHELL—DANGEROUS TYPE

A new type of dam was recently tried and failed. The dam was built across the Niobrara River at Valentine, Nebraska. It was a hollow concrete shell with sand filling. A bed of concrete was first laid over the bed-rock. Sand was piled up on this and the up-stream and down-stream faces of concrete were laid, this construction being carried on until the top was reached, which was entirely of concrete, of course. The dam was triangular in cross-section, with an up-stream slope of about 2 on 3 and a down-stream slope of about 3 on 2. (See *Engineering Record*, April 29, 1911.)

ARCH DAMS

A type of dam called the arched dam is curved in plan with the convex side of the curve up-stream. Quite a number of arched dams have been made; some of them are substantial and strong in appearance, but others do not appear to have the characteristics of permanent engineering works. In *Concrete Engineering,* May, 1910, and in *Engineering News*, Sept. 29, 1910, I called attention to errors held and publicly advanced by designers and builders of these dams. One designer proposed a dam that leaned 20 degrees up stream. The upward pressure beneath it (on the wetted side—not on its base) would be about 12,000,000 lbs., whereas the entire weight was only about 5,000,000 lbs. This same designer elsewhere proposed a dam, part of which was semi-

circular in plan, thus making its end thrust parallel with the rock sides of the gorge. This means that the rock would have to be exceedingly solid, or else a chunk would be sheared out of the side by this thrust parallel with its surface and close to the surface. This is a feature of an arched dam that should not be lost sight of. It is more conducive to stability to have the thrust of the arch forcing into the body of the rock sides than to have it approach a direction parallel with the surface. In the latter condition a portion of the side of the gorge might be dislodged. This designer further says: "The arch—the circular arch, the concentric ring arch—when loaded by water pressure is the most perfectly loaded form into which material can be placed for it is the equivalent of a balanced load on a column without length." Another designer says: "In designing these dams the complex question of the exact stresses that may occur and the assistance that may be afforded by the weight of the wall has been disregarded; as it has been considered that all practical requirements would be met if in theory the dams were treated simply as sections of rigid cylinders subject to exterior water pressure." This designer uses a flat unit as high as 310 lbs. per sq. in. for the concrete.

The following is quoted from my letter, heretofore referred to:

"Why should a circular arch be considered as a short column or a rigid cylinder, with no reduction in the unit for unsupported length? The curved shape of the arch does not insure lateral support; and the water does not supply the lateral support, since the pressure is just balanced by the thrust of the arch. These arches would be in the condition of a thin tube under external pressure, if they formed a complete circle. They are in somewhat similar condition to columns of a length equal to the curved distance from abutment to abutment. It is well known that the collapsing pressure of tubes under external pressure is very much less than that which would give a unit stress to cause failure in a short column. Some formulas based on tests give, for example, in a tube 50 ins. in diameter and 1 in. thick in steel, a collapsing pressure that shows but one-sixth of the compressive strength of the steel.

"One of the dams described has a length at the top more than 200 times its thickness at top. A column of this length would have practically no strength. Others have length greatly exceeding those allowable in columns. It is a good thing that these dams rest on the ground and have the assistance afforded by the weight of the wall, which the designer would disregard.

"Basing unit stresses on concrete in the mass upon small cubical specimens, as this designer has done, is not a very safe method of design. Mass concrete is not uniform, as small blocks would be. Of course a large factor of safety can be made to cover this, but if the unsupported condition of the wall as a column be ignored, the factor of safety may be eaten up by this condition."

DAMS

Part II, written in 1924.

Part I of this chapter has been kept in the form in which it was written in 1911 as a matter of historic record. Since that time a few engineers have recognized the fact that all must eventually recognize, namely, that under pressure is just as essential a consideration in the design of dams as any other force. The subject has been up for discussion before engineering societies. Engineering periodicals have given recognition to the forces produced by under pressure. It is not now standard practice to ignore it altogether. Books give it some notice. However one very recent book on hydroelectrical engineering mentions under pressure as though it were of some academic interest, after devoting many pages to the old-time rules for designing dams which utterly ignore under pressure.

Some notable failures of dams have taken place, and some strenuous efforts have been made to convince the engineering profession that these dams, which can readily be proven to be insufficient in their design to resist under pressure, failed from other causes.

The following is quoted from my contribution to a discussion on the subject of under pressure on dams which is published in Transactions, American Society of Civil Engineers, Vol. LXXV, 1912:

"One of the most momentous questions at present before the Engineering Profession concerns the design of dams; not that it inherently possesses such serious problems, but that the question is a psychological one. When a large body of men is compelled to change any adopted ideas or standards, something must happen. Even in so simple and eminently useful a change as the adoption of standard time in place of sun time bitterness and strife amounting almost to revolution accompanied the discarding of 'God's time.' Many towns for years had two or three standards of time. Thus far it has not been held that the (to date) general standard of designing dams is of divine origin, but something of the same fanatic opposition to discarding it has been heard from many quarters, in spite of the many lives which have been sacrificed to it and by it, as was heard when 'railroad time' was on the rack; and compromises are suggested, as in the other case.

"It is absolutely certain that engineers must revise their methods of calculating the stability of dams; the serious question is whether or not that revision will be thorough and complete, or will it for a season of years be a dangerous compromise to be completed only when another great disaster and the blood of other scores or perhaps hundreds of victims cry out? No future book on dams will commit the awful blunder of all English books written previous to the first failure of the dam at Austin, Pa.; no future book will omit entirely all mention of upward pressure on masonry dams. Surely no revision can be expected to lay adequate emphasis on this omission; and it

is doubtful if many new books will be written on the subject, as it is one which engages the active attention of only a few men. Furthermore, in some quarters, there is a tendency to belittle the real and demonstrable importance of the matter. The writer considers Mr. Harrison's paper an example of this tendency.

"Railroad bridges could be built with no regard whatever for the dead load in calculating the strains. The majority of them would stand up and do service. Some of them would last a long time and never give the least sign of their inadequacy. Some of them, in fact, might never be overstressed, as the assumed live load is usually in excess of any train load. There would be an occasional wreck, but as the fracture of steel is always sharp and crystalline in a sudden break, any required number of 'experts' could be found to prove that the steel was burnt in the manufacture or crystallized in constant service. Just as experts of this class can always find a bad batch of concrete, a shaving, a block of wood, or some dirt in any reinforced concrete failure, so they can detect defective steel or workmanship anywhere.

"If every book on the design of bridges omitted all mention of the need of considering the dead load, and gave examples of designing which ignored the dead load, and if some 'destructive critic' should come along and question the correctness of this, even maintaining that it was positively wrong and wreck-breeding, there would be the same hue and cry against the 'iconoclast' that the under pressure advocate has met; and he would be treated with the same silent contempt from some parties until those same parties had had time to get under cover gracefully.

"It is as erroneous a proceeding to design masonry dams of any class without considering the under-pressure as to design bridges without considering the dead load; but it seems to be more disquieting to many to have accepted standards proven false than to have the Engineering Profession degraded by the periodic wrecks that these same false standards engender.

"The foregoing is preliminary to this thesis: All masonry dams should be designed capable of withstanding upward pressure under the full area of the base the intensity at the up-stream edge being not less than the full head. The writer will go a step farther and say that legislation ought to be passed requiring all masonry dams to be thus built, just as it requires buildings to be designed for dead and live load.

"The wrecks which are occurring with such sickening regularity are writing against the Engineering Profession: '*Mene, Mene, Tekel, Upharsin.*' It is time for the Medes and Persians to come in with some laws which cannot be altered. These three laws would have avoided nearly all the great structural wrecks known:

"(1) Dams must be built to resist upward pressure.

"(2) Structures must be substantially braced during and after erection.

"(3) Concrete shafts must not be considered reinforced when they contain

only slender vertical rods, even if these rods are wired together at wide intervals.

"To be asked for a demonstration of the proposition that water exerts an upward pressure under the base of a dam and in horizontal joints is like being asked to demonstrate that the earth is not flat. To the writer's mind it ought to be all-sufficient if mere mention is made of under pressure as a factor working against the stability of a dam. No demonstration is ever attempted to show that full water pressure against the up-stream face of a dam must be considered, though that face may be largely covered with mud and silt. One might make a paper demonstration that would show that a fillet of mud on the up-stream side of a dam would save a large quantity of masonry, and he would stand on precisely the same ground as the engineer who maintains that under pressure may be neglected.

"Proof of the fact that water will exert pressure wherever it is confined and in communication with other water reaching to a higher head is too puerile to demand attention. Proof of the fact that there is water in the joints of a dam and beneath it, and that this water meets the last named condition, is equally puerile. Someone may say in answer to this, 'capillarity!' Is it capillarity which taxes the capacity of several steam pumps to keep unwatered the foundation of a dam during construction? Is it capillarity which causes water to well up in a spring apparently out of the solid rock? Is it capillarity which carries water a mile or more through the compact earth and causes it to rise in wells or basements to just the height of the surface of a river in the neighborhood? Is it capillarity which causes water to flow out in jets through the joints of a dam, or to seep through the soil beneath a dam and force its way to the surface? Is that water discriminating enough to avoid the base of a dam in its passage? Is it capillarity which causes water to ooze out through a cast-steel cylinder (a little spongy) under great hydraulic pressure, when the same cylinder would be quite water-tight under ordinary high pressure? Is it capillarity which will eventually force out a tightly driven plug in the orifice of a house faucet, if the faucet should leak a few drops a minute? Was it capillarity which forced water under very low head through 30 feet of solid concrete, as shown in the report of the Chief of Engineers, U. S. A., for 1902? (In *Engineering News*, Apr. 2, 1903, p. 306, Col. Peter C. Haines states: 'This showed conclusively that there was less resistance to the passage of water through the 30 ft. of concrete than to its passage through the sandy material forming the earthen portion of the parapet.') Is it capillarity which lifts the waterproof skin applied to a damp wall?"

My contribution to the discussion, from which the above is quoted, gives further facts and experience which show the futility of making a mortar joint water tight, with any degree of certainty, or even of sealing the joint between stone or concrete and the bed rock of a foundation, also the extreme

difficulty of making a concrete wall waterproof, even with modern, poured concrete.

UNDER PRESSURE "SCOFFLAW" FOREVER SILENCED

A recent remarkable example of the action of under pressure in the body of solid concrete that should forever silence the last bitter-ender is published in *Engineering News-Record,* March 13, 1924. A large concrete pier was split almost horizontally, and the upper part, weighing nearly a thousand tons, was raised a distance of fifteen feet. And this is the force that authorities seek to banish by the mere weight of their opinion. Would it have prevented this block of solid concrete from being split in two horizontally if it had been founded on a rock? Would it have had any influence, if the foundation under the lower part of the block, which remained in place, had been grouted to fill in fissures? Would a well-defined and well-intentioned opinion in the mind of the designer that under pressure would not act on this block have held it in place? Or, granting that a layer of laitance allowed to collect during the placing of the concrete through water permitted entrance in this horizontal joint, should the entire safety of a great structure, and the lives of perhaps thousands, depend on the vigilance of one or a few men in the execution of a contract?

In the light of the action of this block of concrete, what about the large proportion of one grain of sand that is in contact with the next grain and that greatly reduces the upward pressure of a layer of water that may penetrate between two layers of sand, as some writers would have us believe?

A short time ago the author of a large book on dams wrote a paper that was read before an international meeting of engineers. The paper was awarded a medal. It belittled consideration of under pressure. One of the principal arguments used was the alleged cross section of a notable dam, the designer of which was said to have neglected under pressure in proportioning the cross section. In criticising this paper, in *Engineering and Contracting,* April and May, 1923, I pointed out that, according to the cross section shown in the paper the dam in question had a base wide enough to give complete stability against full under pressure, and the designer's idea has nothing to do with the stability of a dam—it is the proportions of the dam. The author of the paper stated that the dam was not built according to those dimensions. This is a fair sample of the type of logic that is set up in defense of neglect of under pressure.

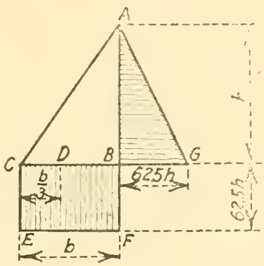
MINIMUM WIDTH OF A SAFE GRAVITY DAM

The demonstration as to the proportions of a triangular dam necessary to resist full under pressure is as follows. (See *Engineering and Contracting,* May 9, 1923.)

Given a gravity dam with a cross section ABC, assume masonry to weigh 150 lb. per cu. ft. and water to weigh 62.5 lb. per cu. ft. The forces against one foot of this dam are the triangle of pressure ABG on the wetted surface and the rectangle BFEC of under pressure on the base. The masonry ABC has a moment about D equal to the weight of a triangle one foot thick with a lever arm one-third of b. Stability is attained if the moment of the overturning forces about D is equal to that of the masonary about the same point. The equation is

$$150\ bh/2 \times b/3 = 62.5\ h \times h/2 \times h/3 + 62.5\ hb \times b/6$$

The solution of this gives $b = .84h$.



Hence if the base of a triangular dam is 84 hundredths of the height, the dam will be completely stable, so far as overturning is concerned, against full under pressure over the entire base. Exactly the same result is found if the pressure is assumed to vary in intensity from full head at the up-stream edge to zero at the down-stream edge.

THE GLENO DAM FAILURE

A few months after the foregoing discussion took place the Gleno Dam in Italy failed and 500 people lost their lives. Millions of dollars worth of property was destroyed. An editor of an engineering paper classes this as among the great disasters of all time. The cause was under pressure in the masonry joints. An engineer on the ground succeeded in setting up specious arguments aiming to show that the failure was due to shear "in conjunction with tension near the upstream face of the buttresses." The unit shears found by him were only a small fraction of the unit shear recommended by America's leading authorities for safe units on reinforced concrete (which I publicly condemned, however) ; these units are 21 to 33 lb. per sq. in. This engineer states that uplift "is considered to be of very minor importance in a dam of the present type." He also states that the "pressure line is within the middle third." Now, if the pressure line were within the middle third, there could be no tension near the upstream face of the buttresses, which, as above quoted, is held by him to be a contributory cause of the failure. In

spite of the light way in which he dismisses uplift he states that "percolation of water through the rubble masonry base may have caused enough leaching out of the lime mortar to weaken its supporting power and simultaneously set up a bursting pressure sufficient to change the stress conditions in the buttresses."

And this "bursting pressure" (uplift) is "considered to be of very minor importance in a dam of the present type." I am led to wonder what sort of pressure would be deemed worthy of consideration by this investigator, in a joint where the mortar had leached out.

The Official Report on this failure, published in *Engineering News,* Aug. 7, 1924, goes far enough to consider under pressure on a portion of the rock base. Nothing is said of the "kiting" effect on the cellular structure, if pressure in the lime mortar joints in the base of the dam be assumed. Years hence it will probably be standard and accepted practice to consider the known under pressure and joint pressure on dams as forces to be reckoned with.

In *Engineering News-Record,* Apr. 10, 1924, p. 626, I proved conclusively by a sketch showing the forces on this dam that the failure was due to under pressure. For one foot of the dam the uplift is a quarter of a million pounds, even assuming that the intensity of this pressure diminishes to zero at the down-stream edge. *When hundreds of millions of pounds of force are utterly ignored for no other reason than the magic in the "type of dam," it is high time that a reformer, an iconoclast, a man who tears down, one with a voice like thunder should appear to protest against the terrible slaughter and waste of property that tramps on the heels of this wilful ignorance.*

FALSE ASSURANCE CONCERNING ARCH DAMS

In *Engineering News,* Sept. 4, 1913, Mr. George H. Moore, in an article on "Neglected First Principles of Masonry Dam Design" states very emphatically that "No arch dam of *any* size, shape or parentage has *ever* failed." The Gleno dam failure has proven that there is no magic in the mere arch form and that this type of structure is amenable to the laws of stability and gravitation. Mr. Moore in his article attempts to belittle consideration of under pressure. He apparently shows by equations that to be stable against pressure varying from full head at the up-stream edge of the base of a triangular dam to zero at the down-stream edge the dam should be 29 per cent wider than if this under pressure were neglected, and if a uniform pressure be taken the increase is 58 per cent. But his calculations are in error, since he takes the center of moments at the down-stream corner. This would mean an infinite unit pressure on that corner. The proper center of moments is one-third of the base from this corner, and it will be seen that whether the upward pressure be considered uniformly varying or the full head over the entire base the moment is exactly the same.

In *Engineering and Contracting* in 1914 some discussion on the failure of dams was published. In the issue of June 17, 1914, a letter of mine appears which answers some of this discussion and makes some suggestions as to the cause of failures of some earth dams or levees. Parts of this letter are quoted below:

"Prof. H. P. Boardman quotes my statement that 'no dam ever failed that was heavy enough and wide enough at the base to be stable under the pressure of water on the wetted side and underneath it, regardless of what it was founded on.' He seems to interpret my meaning to be that dams ought to be built regardless of what they are founded on. But there is no need to take this meaning. My letter was written to contrast the importance of *design* as compared with *foundation*—to emphasize the fact that failures that have been commonly and uniformly attributed to foundation are in reality due to faulty design, and there is no reason to anticipate stability in a dam when the very design does not take into account the possible pressure on it.

"A reinforced concrete structure might be so fragile that it would fail by reason of a very slight settlement in one of the footings, and investigators might blame the whole failure on the foundation. But what about a design that lacks reinforcement to give the structure toughness? Why blame the foundation for doing the thing that all foundations, not of solid rock, always have done and always will do, namely, settle a little? Why blame the soil under a dam for doing what soil always has done and always will do, namely, wash away when a stream of water flows over it? When the pressure under a dam exceeds the weight of the dam, it will lift the dam and allow a stream of water under heavy pressure to flow over the soil, if the dam itself is not carried away. This erodes the soil and undermines the dam. If the dam had been heavy enough to keep its bed, the water would not have created a cavity and started a flow. In hollow dams like the Stony River and the Pittsfield, because of the reinforcement, the superstructure is apt to hold together, for a time at least, until the base is undermined. In mass concrete dams of insufficient width of base, like the two Austin dams, the wall breaks up and is at once pushed down-stream by the impounded water.

"'Facts' have been called for on this subject. The quotation from my letter, in the second paragraph of the present letter, is a fact, and a fact of some significance. There are dams as wide as 0.85 of the height, but none of them have ever failed. The solid masonry dams that have failed have had bases about 0.65 of the height, for this is the theoretical width for dams designed regardless of under pressure. Is it throwing away money to make the width of base 30 per cent more and make something that water cannot lift? This does not mean 30 per cent added to the cost of a dam, for a dam of proper width does not of necessity require the cut-off walls that have 'safeguarded' the life of so many dams (many of the failures have had one or two of them). Furthermore, the cost of a dam is not directly proportional

to the amount of masonry. In any event, would it not be good engineering to spend even 30 per cent more for a dam if this means the difference between failure and safety?

"Here are some more facts. They are taken from 'Concrete Age' for May. A concrete tank at the foot of Walrous Ave., Tampa, Fla., 10 ft. square and 13 ft. deep, was made in the ground, and earth was tamped around it. The walls were 13 ins. thick, and the bottom was 16 ins. thick. The weight was then about 90,000 lbs. This heavy tank was completed on Saturday and was lifted over Sunday by water that seeped beneath it and *raised 47 ins.* It would take just about 13 feet of head of water to lift this tank if it were entirely free of friction on the sides, and if the water had unhindered access to the entire area of the base. Are these not the very facts that your editorial in the issue of April 1, p. 376, states that the profession needs? On page 600 of the issue of May 27 the same need is reiterated.

"Prof. Boardman states that 'in a general way' it is a crime to design a dam without considering under pressure, and then he takes all of the force out of his assertion by saying that all of the 'considering' that is necessary is to take a good look at the foundation and squirt in some grout, then make a cut-off wall that will cut off. But underground water is not controlled by such simple means as this would seem to call for. Very often concrete tanks that are built with care to retain water, built up in the air with every advantage of easy access, hold that water like a sieve. When engineers want to make sure of water being kept out of the basement of a building, they usually paper the entire ground with several thicknesses of tarred felt. Did you ever hear of a dam builder papering the entire bottom of a lake or stream? Cracks, both vertical and horizontal, in a concrete wall are very common. What if a crack should develop just above a carefully examined and grouted foundation? What if the cut-off wall should crack away from the main dam?

"Prof. Boardman mentions the Hauser Lake dam as another failure due to foundation. I have four letters in my files in which I warned the engineer of this dam during its construction of the danger of the *design.* It happened that I was consulted concerning features of the steel deck. I looked farther than the steel deck and found that a high apron exerted an unresisted uprooting pull on a row of sheet piles at the foot of the slope of the steel deck. In order not to be connected with the failure, which I anticipated, I did what I could to avert it. This dam failed exactly as I predicted it would, but my warning was ignored.

"Here are some more facts that have a bearing on under pressure in dams. In *Engineering News*, June 4, 1914, there is a description of an earth dam that failed on the upper Sevier River in Utah. It is stated that 'large sections of the lower face began to slip off.' Evidently the water was just beneath this lower face, the hardened compact surface of the down-stream slope. The theory is here set forth that most failures of earth dams are one

piece with those of masonry dams. The under pressure in the earth dams is exerted by a sort of semi-liquid mud that is formed by the saturation of the interior of the dam. The natural and apparently reasonable thing to do is to compact the down-stream slope, sometimes to pave and grout it. But this is the very thing that is apt to cause the entire dam to fail. Instead of the water near this surface being allowed to escape, thus drying out the interior of the dam, it is held in until the saturation of the interior is complete. This semi-liquid mud then exerts a hydraulic pressure on the hardened crust and flows out, resulting in failure of the dam. A full height core wall and little or no compacting on the surface would seem to be the remedy. Possibly the planting of grass would be a great aid, as this helps to retain the porosity of soil. Also roots reach down and take up moisture from below the surface. I think this would make a good subject for discussion by engineers engaged in building earth dams.

"Here are some facts that substantiate my theory. In *Engineering Record,* Apr. 29, 1911, there is a description of the failure of the earth dam of the Julesburg reservoir. It was clearly evident that this dam failed by under pressure of the water, and the article so states. The overlying mass of rock and earth on the toe of the slope was lifted by the pressure of the water. The remedy here applied was to introduce a core wall in the rebuilding.

"In the same issue of the *Record* there is a description of another dam that failed. This was a hollow dam of mass concrete filled with sand. It was very broad in section, nearly like an earth dam. But when water was let in, the dam failed. Evidently the failure was by access of water to the sand interior and pressure that lifted the down-stream layer of concrete.

"Prof. Boardman says, 'If there were a stratum of sand below the base of such a heavy dam and the water in the reservoir gained access to it and also gained exit below the dam, it would probably not take any longer to undermine the structure than if it were an earth fill or a hollow concrete dam. The result would be, no doubt, a worse wreck than occurred at the Pittsfield dam wash under.' He also cites failures in levees which started by 'seeps' or 'high water springs.' In making the above statement and these citations Prof. Boardman fails to consider the fact that in a properly built earth dam there is a great barrier against the flow of water in the core wall which does or should prevent saturation of the soil under the down-stream slope: this is not found in the levee. In the 'heavy' masonry dam the barrier is the broad base of the dam and pressure that resists the uplift of the water: the hollow dam does not have this pressure, hence the water lifts it and forms its own cavity where the exit is free. It is against all reason that under the broad bases of the hollow dams that have failed the water could inaugurate a course of free flow that would carry away the soil, unless it could first lift the overlying slab. Frozen weep holes would be exactly the condition that

would enable the water to lift this light shell, and both of the dams already referred to failed in mid-winter.

"Springs in soft soil have existed for generations, but they carry little or no sediment. It is when a saturated subsoil under a hard crust breaks through that crust that trouble may be expected."

In *Engineering and Contracting* May 22, 1912, there is a re-print of an article by Mr. Edward Wegmann (to which the editor has added). The cross sections of 40 existing large dams are shown. Many of these would not, if straight, satisfy the conditions necessary for stability, if under pressure were considered. But in practically every such case the dams are curved in plan. (It is not stated whether they are straight or curved in the doubtful cases.) A curved dam while it may not have all of the requirements of a true arch is not in the case of a simple gravity dam since the curve adds to its stability and the difficulty of lifting and overturning it. Mr. Wegmann states that in the case of four dams the designers made no allowance for under pressure. Now if the base is about 85 hundredths of the height, the dams are stable against under pressure in spite of what mental process the designers may have gone through in their design. By Mr. Wegmann's own figures the Furens dam is .94 of the height in the width of its base, the Ban dam is curved, and the New Croton dam is 80 hundredths of the height at the base or 86 hundredths, if the line of the "overfall" be considered the top of the dam. The other dam I cannot connect with any of the figures in his paper. These facts are mentioned to drive home the fact that it is impossible to construct a logical argument that justifies neglect of under pressure in the design of any dam whatever. The scores of failures are weeding out or have already weeded out the examples of dams that calculations show are unstable when under pressure is considered.

LONG LIFE NO PROOF OF STABILITY

Sometimes structures stand for many years, sometimes they fail the first time they receive a critical load. This is true of dams as well as other structures. The Hauser Lake dam failed the first time the water rose high in the lake which it formed. Mr. W. P. Snow tells of a dam at Roxbury, Vt., (*Engineering News,* April 9, 1903, and June 4, 1903) which stood for 30 years and which in that period he had observed to have water 12″ deep on the crest; it failed with water 3 or 4″ deep on the crest.

A gravity dam built in 1860 failed fifty-two years later. This was the Owasco Lake Dam, the failure of which is described in *Engineering Record,* Apr. 27, 1912, p. 476. For half a century this dam could have been pointed to as a case that defied theory or that proved that under pressure does not need to be considered in the design of dams.

It is easy to probe into the soil where the tremendous force of the full pool of water has washed out cavities and find the fissures that this flood of water

has created and to build a theory on this *result* to supply a *cause* for the failure in the *foundation*. This has been done too often, and facts of poor design have been obscured.

FAILURES OF DAMS VERY COMMON

The year 1912 seems to have been a year when there were an exceptionally large number of engineering failures. The following is quoted from an editorial in *Engineering News*, Nov. 21, 1912: "In the six days from Oct. 26 to Oct. 31, five fair sized dams in this country failed, in such widely separated states as New York, Washington, Nebraska, Ohio and Colorado. In one month last spring five dams went out in one county in the State of New York. These are exceptional cases, but hardly a day goes by that does not bring to this office newspaper clippings telling of the failure of a dam. We are convinced that taken as a whole there is more (disregard of engineering principles in the design and construction of dams than in any other structure) and that this disregard results in more damage to property and loss of life than can be laid to the combined failures of bridges, buildings, piers and other static structures."

DESIGN OF DAMS RECOGNIZED ON UNSOUND BASIS

Mr. Lars Jorgensen, in *Engineering News*, Sept. 25, 1913, p. 624, says, "The writer has in his file the records of 98 dam failures, collected largely from information published in engineering journals, and a study of these records shows that nearly all gravity-dam failures have been due to sliding or overturning." This record would not be possible if the design of dams were on a sound basis.

If the sporadic efforts on the part of editors of engineering periodicals exhibited on occasions after a large number of failures or after a very disastrous one were followed up by frequent reference to the need of reform and urgent demand for such reform, the thing might even get into standard books when they are written or revised.

In 1914 the Stony River dam failed (See *Engineering News*, Jan. 22, 1914; *Engineering Record*, Jan. 24, 1914). This was a hollow dam totally inadequate to resist under pressure. It failed in winter when the weep holes in the horizontal slab were doubtless frozen up. Of course soil was washed out under the dam, and of course reports of the failure described it as a failure of the foundation. This dam was rebuilt later, and a paper was read before the American Society of Civil Engineers on the subject of this reconstruction in 1917. I attempted to exercise the privilege that members, and even strangers are accorded, in submitting a contribution to the discussion. I was refused this privilege by the publication committee. (The committee acting that year also refused to allow me to discuss the Report of the Joint Committee on Concrete and Reinforced Concrete of which I was a member.)

The author of the paper on the reconstruction of the Stony River dam stated in his paper that the failure of the dam had not been due to the type of dam. And yet in the reconstruction the type was changed (1) by adding a large amount of concrete near the upstream edge of the base, where it would do the most good, (2) by anchoring the deck slab to the cut off wall, a feature not in the original dam, (3) by adding a very elaborate drainage system with perforated pipes set in the ground, and (4) finally by housing in the outlets of the drainage system to prevent freezing.

THREE HOLLOW DAMS FAIL WHEN WEEP HOLES ARE CLOSED

It is significant to record that while the Stony River dam and the Pittsfield dam failed in mid-winter when the weep holes were doubtless frozen up, another hollow dam failed when the city authorities drove wooden plugs in these weep holes to conserve water. This was the Plattsburg dam, described in *Engineering News,* June 8, 1916, p. 1106, another hollow dam that failed.

MANY EARTH FILL DAMS FAIL

Earth fill dams have already been referred to, and attention has been called to the possibility of water penetrating under a down-stream pavement or a hardened crust and softening the soil beneath and the pressure of the water lifting the pavement or crust. Two notable failures of earth and rock fill dams seem to teach the same lesson concerning dams of this kind. In *Engineering Record,* Sept. 16, 1916, Mr. Ralph Bennett tells of the failure of an earth dam with a thin core wall and slopes on both up-stream and down-stream faces of 1½ to 1. In *Engineering News,* Feb. 3 and 17, 1916, and *Engineering Record,* Feb. 12, 1916, the failure of the Lower Otay Valley dam is described. This dam also had a thin core wall and slopes on both up-stream and down-stream faces of 1½ to 1. The core wall had a steel diaphragm. In *Engineering Record,* June 10, 1916, Mr. George S. Binckley says, "The Otay dam cannot be considered as an example of engineering design or even good construction." The dam impounded water from a drainage area of 100 square miles, and the spillway was only 38 ft. wide with its lip only 3 ft. below the top of the core wall. In *Engineering Record,* Aug. 14, 1916, p. 340, we are told that the failure of this dam was declared by the courts to be an Act of God.

The Dells dam, near Hatfield, Wis., was an earth dam with a much longer slope on the up-stream face than on the down-stream face, and with thin core walls. It was paved with rip rap. It also failed (See *Engineering News,* Oct. 19, 1911). The Hebron earth dam at Maxwell, N. M., failed. The up-stream slope was 3:1, and the down-stream slope was 1½:1. (See *Engineering Record,* May 30, 1914.)

The stability of an earth dam is not a thing that can be worked out to the degree of accuracy that obtains in masonry dams, but there is a feature common to the four dams above referred to that is susceptible to analysis that would throw doubt on the stability of the dams. This feature is not referred to in the published comments on these failures as being responsible for the failures. These four dams had thin core walls; two had equal slopes on up-stream and down-stream faces, the others having a flatter slope on the up-stream face. Now it is quite evident that the pressure on the up-stream face of the core wall, due to the earth fill and the water is more than that of the earth fill alone on the down-stream side of the core wall. That this pressure would move the core wall when water is not near the height of the crest is not probable, but with high water this unbalanced pressure is very apt to push the core wall down-stream and dislodge part of the down-stream fill. This doubtless had a large influence in the failure of these dams. The remedy is a very much flatter slope on the down-stream face and a core wall spread out to give some stability against the unbalanced pressure.

The failure of the Apishapa earth dam in Southern California is described in *Engineering News-Record,* Aug. 30, 1923, and Sept. 13, 1923. This dam had very low core or baffle walls, but the up-stream slope was 3:1 while the down-stream slope was 2:1. The up-stream slope was paved with rip rap, but this would not lessen the penetration of water. The dam failed with the water level 10 feet below the crest. If a plane be imagined through the middle of this dam, it will be seen that the wide, flat slope on the up-stream side and the full pressure of the impounded water will be very much greater than the pressure that could be actively exerted against the same plane by the fill on the down-stream side of the plane with its steeper slope and very much smaller quantity of material. It is said that there were cracks or fissures in the dam before the failure. These were doubtless caused by the great unbalanced forces that these conditions would anticipate.

The paving of the up-stream slope of an earth dam with a continuous reinforced concrete slab would add greatly to its stability because it would diminish the quantity of water that could soak into the earth. The pressure on this pavement would be a downward force easily resisted by the earth fill, and its effect would be to compact the fill. It is important that this pavement be prevented from slipping or undermining.

The Dalton dam at Mineville, N. Y., failed. (See *Engineering News,* May 9, 1912.) This was an earth dam about 32 feet high with a core wall 5′-6″ wide at the base, the wall being reinforced with steel cables. The slope on the up-stream face was 2:1 and that on the down-stream face was 2½:1. In these respects the embankment was good; but the core wall should have had rod reinforcement, as steel cable is not a good reinforcement for concrete. If conditions at the end of the embankment had been properly taken care of, this dam would doubtless not have failed. At one shore the

core wall was joined to a thin wing wall built in sandy soil and apparently without a base course. The failure started at this wing wall, which was at a sharp angle with the core wall. The sandy soil was washed out, and the embankment raveled, as it were, from this weak point. The water carried away earth fill and the greater part of the core wall.

The Mohawk dam at Tiffin, O., had both slopes paved with concrete. As would be expected, the dam failed—upward pressure on the down-stream paving. (See *Engineering News,* June 10, 1915, p. 1121.)

Earth fill dams seem to be peculiar to America. Hydraulic fill dams, at least, are unknown in England according to a statement by Mr. Martin Deacon, Assoc. M. Inst. C. E. of Westminster, England. (See *Engineering News,* Sept. 22, 1910, p. 306.) The history of earth fill dams is not one that can be pointed to with pride by engineers, for a large number have failed.

In the accounts of these failures, though many of them are very voluminous, there is frequently a paucity of the most essential features of design. In one long account of the failure of a large earth-fill dam mention was made of pavement on one slope of the dam which was said to have slipped and under which water pressure was said to have acted. I was unable from the account to tell whether the paving was on the up-stream or the down-stream slope. In another account a spillway was described as being paved on both slopes so many hundred feet each way from the center of the dam. The center of the dam was not defined, and it was impossible to do more than guess whether or not it was in this spillway that the failure occurred.

Many accounts of earth fill dams that have failed tell of previous experience when leaks occurred on the down-stream slope and that these leaks were stopped. Few of these accounts tell how these leaks were stopped. This is of the utmost importance. When leaks of any consequence occur on the down-stream slope of a dam, the dam is sick and needs heroic treatment. The compacting of the soil or the plugging up of the holes with boulders simply makes things worse. It is like sealing up a sore that should be open to drain. Compacting the soil on the down-stream slope or plugging rocks in the holes will stop the loss of water for a time but the water that would leak away is thereby forced to accumulate pressure and to soften more of the underlying mud, and the accumulation of this pressure in semi-liquid mud is what has started the failure of many an earth dam, for the pressure lifts the overlying hardened soil or rocks and the mud and water flow out.

The Hatchtown dam failed. (See *Engineering Record,* June 27, 1914.) In the case of this dam seepage had been observed years before, and some sloughing. The account says, "A heavy bank of rock was piled against the slope of the dam along the line where the sloughing occurred, while the water

was still at its maximum height in the reservoir, and the sloughing tendency was entirely checked." The escape of pus from the wound was thus stopped by external bandages—for a time.

The material under the down-stream slope of a dam should not be compacted for the purpose of rendering it watertight. If water passes a plane of the dam passed vertically through the crest, it should be allowed to get away. This is why puddle core walls and concrete core walls are built. All of the compacting (to render fill waterproof) and all of the water seal should be confined to the core wall and the material under the up-stream slope. Of course the material under the down-stream slope should be compacted enough to make it as solid as possible, but it should be free from clay and as porous as possible so that it will not hold water or turn to mud. The down-stream slope should not be paved, unless the paving is thoroughly under-drained, with an outlet that cannot freeze up (under water).

CHAPTER II

ARCHES

UNSATISFACTORY STATE OF THEORY

The theory and practice of designing arches, particularly reinforced concrete arches, are in a very unsatisfactory state. There is entirely too much theory and too little common sense. There are many tests made, ostensibly to substantiate theories, on models that are far from being representative of actual conditions. Abutments of great mass, rigidly held in place, with slender arch rings, are used in tests, and these tests are blandly held out as a basis for the design of "elastic" arches.

Is it any wonder that designs made on the basis of such data as these tests should produce arches that crack in service and sometimes fail and must be replaced?

NEGLECT OF ABUTMENT IN DESIGN

The stresses in an arch ring are given minute consideration by these theorists, using the elastic theory with all of its complication, while the abutments of these same arches are given almost no consideration. Arches are considered absolutely fixed ended, when they are to be sprung from abutments that are far from rigid. In fact the rigidity of these abutments is given no thought. Often even stability against overturning is ignored or merely postulated on assumptions of conditions that do not exist. (Active horizontal pressure of earth, for example.) The result is that many arches that would be pleasing in appearance are found with unsightly cracks, not to mention those that have failed through insufficient abutments.

BASKET HANDLE OR SEMI-ELLIPSE IMPROPER SHAPE FOR ARCH

So-called basket handle arches, arches with a sharp rise at the abutments, are illogical in shape. Any arch with high and narrow abutments or with a sharp rise near the ends is wrong in principle, unless it be for a sub-aqueous tunnel or a tunnel with rock sides; for to preserve the equilibrium of such an arch there must be horizontal pressure exerted against the arch by the fill. Horizontal pressure may be exerted by the earth and it may not. The case of retaining walls is often cited in attempts to prove that earth exerts a horizontal pressure. The pressure against retaining walls is chiefly due to three causes: first, the expansive force of frost; second, the hydraulic force of

semi-liquid mud; third, the sliding force of loosened blocks of earth. The idea of earth acting as a granular substance, upon which many fine-spun theories of earth pressures are based, has scarcely any existence or basis in fact, except in the case of dry sand, a material that requires patient cooking in pans to obtain in small quantities.

DESIGNING FOR ASSUMED HORIZONTAL EARTH PRESSURE DANGEROUS

Analyzing these three sources of pressure of earth, it is seen that none of them is active except under certain conditions. It would not do to design an arch whose stability depended upon the expansive force of frost, for this acts only on rare occasions. It would not do to depend upon semi-liquid mud, for this will dry out. Even when the retaining wall is not yet built, earth will very often stand vertical with no shoring whatever. This is positive proof that such earth exerts no active horizontal pressure, and it nullifies completely the fine-spun theories of earth pressures. Of course blocks of this earth may be loosened and may slide out with great force when once they are so loosened. Such a contingency could not be made an element in the stability of a properly designed arch.

It is true that earth does exert a lateral pressure when greatly overcharged or heavily loaded. It will tend to flow laterally under such conditions. The fill over an arch, however, does not present these conditions. This fill could in most cases be supported by vertical props alone with lagging in steps surmounting the same. This alone is ample demonstration that the horizontal pressure in such cases is non-existent.

Assumption of horizontal pressure in the fill of an arch greatly reduces the amount of masonry apparently necessary in the abutments, and by this very fact creates a false notion of stability. There is scarcely any doubt that in the many examples of arches cracked at the haunches from settlement or spreading the failures are due to this erroneous method of design. The remedy is obvious, namely, to design the arch on the assumption that the load is purely a vertical one.

Another fault in design exemplified in the failure of arches is the failure to recognize that every arch must have an abutment or at least a sufficient and constant force exerted against it to take its full thrust. This is so simple a proposition that everyone who attempts to design or build an arch ought to be thoroughly familiar with it. And yet some arches are built, and some series of arches, where this principle is ignored, and failures have been the result.

GROINED ARCHES FAIL BECAUSE OF IMPROPER DESIGN

Groined arches in reservoir roofs, being in series and resting on post, must

depend upon one another for resistance to thrust up to the outer arches in the system, which must of course be supplied with abutments. In the construction of these arches it is necessary to bear in mind that the middle arches lack abutments. Forms should not be removed from outer arches until they join up with the abutments, even though the concrete may be thoroughly hardened. Several reservoir roofs have failed because this precaution was not taken.

A groined arch filter roof in Philadelphia failed, as described in *Engineering Record*, Jan. 24, 1914, and *Engineering News*, Jan. 15, 1914. The abutments for the outer arches were insufficient. Cracks in the outer arches had been observed years before the failure. Dependence upon active horizontal earth pressure to resist the thrust of the outer arches probably had much to do with this failure.

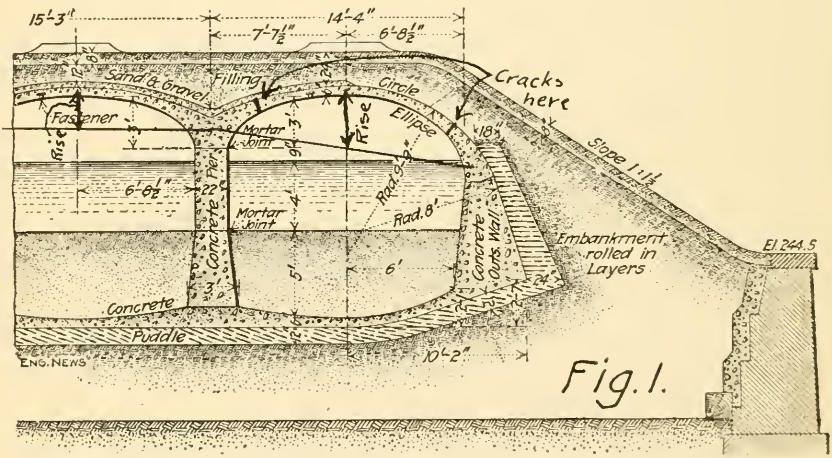


Fig. 1.

Figure 1 is a cross section of this filter roof. It is seen by this sketch not only that the abutment of the outer arches is rather light but, what is more significant, the effective rise of the outer arches is much greater than that of the interior arches. The effect of this condition would be to force the outer arches upward and break the same, due to the inability of the outer arches to resist the thrust of the inner arches. Two arches of the same span and loading will have thrusts inversely as their rises, hence the outer arches in this construction will exert a thrust much less than that required to maintain equilibrium in the interior arches. These facts were not pointed out in a long account of this failure. They are the real and about the only lessons of the failure.

The failure of the groined arch filter roof at Baltimore, Md., described in *Engineering News*, Nov. 27, 1913, and in *Engineering Record*, Oct. 25, 1913, is an example of the lack of provision against horizontal thrust of arches

already made and prematurely loaded with earth fill. Fig. 1 on page 1102 in the issue cited shows clearly this lack. Outer arches were loaded with earth fill and their thrust would be exerted against forms for inner arches merely supported on the posts, the inner arches not having been poured. Hooped columns instead of the rodded columns used might have saved this structure.

SHORT SPAN WITH HIGH RISE CAUSES FAILURE

Another example of a failure of a series of arches is found in the collapse of the reinforced concrete bridge over the Flat Rock River near Edinburg, Ind. (See *Engineering Record,* March 12, 1910, and *Engineering News,* March 17, 1910, Apr. 21, 1910, and May 12, 1910.) In this bridge there were three arches in a series. The middle one, with a 90-ft. span, had a rise of 9 ft. and the outer ones, with 75-ft. spans had each a rise (effective) of 11 ft. The piers between arches were narrow and not designed to take much thrust. The astounding feature of this design is that the long span had the low rise and the short spans had the high rise. In consequence of this the excess thrust of the middle span forced up one of the side spans. This side span then dropped, relieving the end thrust of the middle span, which then collapsed: this was followed by the collapse of the other side span.

The designer claimed that the cause of this wreck was poor foundation of the piers. Whatever ground there may be for this contention it is certain that there is ample reason to expect a failure exactly like this in a bridge on good foundations designed on these lines.

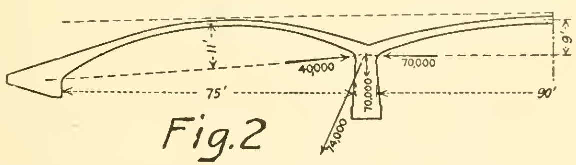


Fig.2

Fig. 2 (from *Engineering News,* Apr. 21, 1910) shows a longitudinal section of this bridge and the approximate thrusts due to dead load on one foot of width. The longer arch with the smaller rise has of course a larger thrust than the others. The unbalanced thrust has to be resisted by the pier. The resultant of the unbalanced thrust and the weight of the pier is a force of 74,000 lbs. which does not strike near the base of the pier. The pier, it will be found from the data, could take about 10,000 lbs. of the unbalanced thrust, leaving 20,000 lbs. of horizontal thrust to be taken by the arch. This is a force that the arch ring itself is altogether incapable of resisting. The spandrel walls must have been doing heavy duty before the collapse. It is a wonder that the bridge stood up any time after the centers were removed.

The designer of the bridge of course tried to controvert the above reasoning, which was contained in a letter to the *Engineering News.* By arbi-

trarily shifting forces and ignoring others he appears to show that equilibrium ought to exist. He says, however, "If the arches had been built of stone with no reinforcement, as shown in Fig. 2, failure would have resulted." This means that the structure was not meant to act as a series of simple arches, even for dead load, but as a combination of arches and cantilevers, the forces being arbitrarily located by the designer.

TRUE RISE OF ARCH WITH ABUTMENTS ON DIFFERENT LEVELS

The difference between the elevation of the crown and the higher end of the arch at springing is by no means the rise of this 75-ft. span. If a horizontal arch is raised at one end, the vertical ordinates of the curve remaining the same, the horizontal thrusts remain exactly the same, and the vertical reactions on the two supports will equal the load between each respective support and the high or horizontal point of the arch. This is true even when the arch is brought to a position that makes it horizontal on one support. But it is seen that the vertical load on the high support is less and less as that support is made higher and higher. Thus the stability of the high pier is diminished by dropping the springing at the abutment, because the vertical load is less. This is just the case in the side arch of this bridge.

Doubtless no engineer would deliberately design three arches in a series such as this with the high rise in the short arch, if the abutment were on a level with the pier; and doubtless the designer here believed that the rise of this side arch for calculations of the thrust was the difference in elevation of the right end and the high point of the arch. But this is not the case. The effective rise of an arch of this sort is the vertical ordinate from the center of the chord to the curve.

The shorter arch with the higher rise and practically the same load per foot will have a thrust less in amount than the other, depending upon the squares of the spans and inversely as the rises. Hence the necessity, in a series such as this, of having a lower rise in the shorter arch.

SKEW ARCHES

In March, 1910, Mr. C. R. Young read a paper before the Canadian Cement and Concrete Association, at London, Ont., on failures of concrete bridges. (See *Engineering Record,* Apr. 16, 1910.) One of these failures was that of a Monier arch of considerable size and built at an extreme skew. The location was Bendigo, Australia. The abutments were especially massive and securely founded on solid rock, and the materials were excellent. It was evident that the design of the arch ring was at fault. Prof. W. C. Kernot, of the University of Melbourne, made some tests on models which showed that Rankine's theory of the skew arch was altogether erroneous. Rankine states that "the forces which act on a vertical layer or rib of a skew arch

with its abutments, are the same with those which act in an equally thick vertical layer of a symmetrical arch with its abutments, of the same dimensions and figure, and similarly and equally loaded."

Without making experiments or tests or even calculations it would seem, from a mere inspection of the plan of a skew arch of monolithic construction that the tendency would be for the thrust to take the short course between abutments. This would mean that the principal thrust of the arch is at right angles to the axis of the arch barrel and not parallel with the axis of the bridge. The result of this tendency is to concentrate the thrust in the part of the arch between A and C, Fig. 3. This is just what happened in the arch at Bendigo, for the portion enclosed by the dotted line in Fig. 3 (which is a sketch of that arch) broke out and the abutment crushed in the double hatched portion at C.

In defense of Rankine's theory of the skew arch it must be said that it is scarcely possible that he contemplated the application of it to a monolithic arch but only to arches of brick and blocks of stone. The latter forms of construction, in ribs parallel with the axis of the bridge, would act quite dif-

Fig. 3.

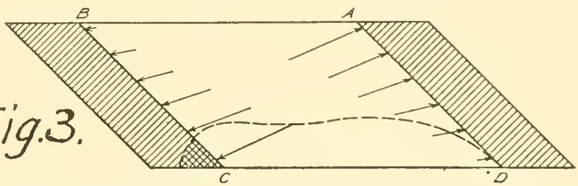


Diagram Showing Cause of Failure of Concrete Skew Arch.

ferently from concrete or reinforced concrete arches. In monolithic arch rings the ribs, of which the arch is assumed to be made up, cannot deflect independently, hence the tendency for the thrust to take a different course. In arches made of blocks, if there is not cross bonding, the several rings or ribs are freer to deflect independently.

REINFORCED CONCRETE SKEW ARCHES TO BE AVOIDED

The lesson from this failure is that monolithic skew arches must not be designed on Rankine's skew-arch theory. Perhaps the best solution of the skew arch problem in concrete is to build the arch in separate parallel ribs not bonded in such manner as to transmit shear from one to the other.

BASIC ASSUMPTION OF THEORETICAL TREATMENT UNSOUND

A paper by Mr. J. Charles Rathbun was read before the American Society of Civil Engineers, Feb. 6, 1924, on the analysis of the stresses in the ring of a concrete skew arch. The analysis is extremely complex. The paper contains about 400 integral signs, which indicates the mathematical maze

through which anyone using the methods of the paper would have to travel. The whole intricate mathematical structure of this paper rests on the soundness of the basic assumption concerning skew arches, and it must stand or fall according as this assumption is sound or otherwise. The basic assumption is that the abutments of the skew arch are perfectly fixed ended. This assumption can be shown to be unsound both from the standpoint of analysis of the conditions attending arch construction and from the result of experience.

In ordinary structural frames or girders small departure from absolute fixed-endedness does not greatly affect the resulting moments and stresses, but in the case of skew supports, departure from fixed ended conditions works a large change in the disposition of reactions. The change is such that the nature of the reactions is completely altered. The skew arch that is fixed ended will, according to Prof. Rathbun's paper, resist the thrust caused by the arch load to the extreme acute corner of the skew with the same intensity as it is resisted in the obtuse corner. If this be true, it is of utmost importance that we examine the conditions and see whether they are such as to insure unvaryingly absolute fixed-endedness; for experience and ordinary common sense tell us that the thrust of a skew arch will take the nearest course to the abutments, and this would throw the greater part if not all of it close to the obtuse corner.

In the first place absolute fixed-endedness of abutments is a thing not practically attainable, in spite of the large amount of matter in engineering literature on elastic arches based on this slender and impossible assumption. It was, however, practically attained in some tests on toy arches securely anchored with heavy anchors to the basement walls of a school building, and on the results of such tests large structures are designed. Soil is not rigid: it will deflect in response to pressure. To furnish the stability to resist without deflection the great end moments of a fixed-ended arch of any size would require massive abutments such as are not provided in any ordinary design.

I am in accord with Prof. Rathbun's earlier views expressed in a letter to the *Engineering News-Record,* Apr. 12, 1923, p. 682, where in commenting on the failure of a skew arch at Tacoma, Wash., he says, "By means of models it has been repeatedly shown that the thrust is greater at the obtuse corner (in plan) than at the acute," and later in the same letter, "Waddell, in 'De Pontibus,' says, 'A skew bridge is a structure the building of which should always be avoided when it is practicable.' After a great deal of study on the theory of the skew arch the writer has come to the conclusion that Waddell was right, even if the remark was written before the days of reinforced concrete."

Prof. Clyde T. Morris, in *Engineering News-Record,* Apr. 20, 1922, p. 638, says that tests on a skew arch in a laboratory show that the greater part of the reaction is close to the obtuse corner. In some cases the reaction at

the acute corner is negative. Prof. Morris points out that Hool in *Reinforced Concrete Construction,* Vol. III, p. 43, says, "Skew arches may be treated exactly as right arches, the span being taken parallel to the center line of the roadway and not at right angles to the springing lines of the arch." The unsoundness of this has been attested both in laboratory tests and in experience with skew arches.

FULL SIZE TESTS PROVE UNSOUNDNESS OF BASIC ASSUMPTION IN ALL ELASTIC ARCHES

As a further and a recent light on the unsoundness of the assumption of fixed endedness in arches, either for elastic square arches or for skew arches, it is pointed out (in *Engineering News-Record,* Jan. 24, 1924, p. 159) that in a discussion before the American Society of Civil Engineers, Jan. 16 and 17, tests on a full-sized arch bridge (119 ft. span) proved that the rotation of the piers is perceptible though the piers are founded on rock. The significance of this fact, based on tests on full-sized structures is tremendous. *It means that a large amount of very intricate theory, taking up large space in standard works and the proceedings of engineering societies is utterly worthless.* I have been publicly making this claim for seventeen years. The elastic theory of reinforced concrete arches is based on a premise that is totally false in its conception, which involves absolute fixed-endedness in the abutments. All designs made on the elastic theory are wrong in just the proportion that the dimensions and sizes are influenced by the dictates of this theory.

IDEAL EXAMPLE PROVES UNSOUNDNESS OF THEORETICAL ASSUMPTION

The Bendigo arch in Australia, as has already been pointed out, had specially massive abutments and was securely founded on solid rock. This is unusual, but in spite of these apparently ideal conditions, matching Prof. Rathbun's theory as nearly as anything terrestrial could, the thrusts of this arch took the short course and crushed the abutments in the obtuse corner.

If an arch could be poured in glass and annealed and securely anchored in an unstressed condition to massive and immovable foundations, it would doubtless have the stresses that theory dictates. But forms settle, concrete shrinks in setting, and no designer has a right to hide behind the theory of perfect elasticity when horse sense would tell him to design his arch as a row of blocks, hinged ended, and then anchor the arch into the abutment for additional safety, and to avoid skew arches as such no matter how pretty the theory looks.

In discussing Prof. Rathbun's paper in Proceedings, Am. Soc. C. E., Vol. L, p. 559, I recommended that skew arches, as such, be avoided, and if the

arch must have the appearance of a skew arch, that the construction be "camouflaged." The following is quoted from that discussion: "In order to obviate this type of skew arch, and therefore, avoid all the complex theory in its solution, it is only necessary to design the arch in separate parallel rings or ribs that can act as independent arches as regards main arch stresses. In the writer's judgment this is the proper thing to do. Flexible ties (say rods passed through pipes embedded in the intermediate ribs and fully anchored in the outside ribs) could be used to tie the skew arch together and obtain lateral stability. In order to give a smooth or cylindrical surface on the intrados of the arch, a false ceiling could be hung under the arch, or the under side of each arch rib could be made to conform with the cylindrical surface. The upper side, or extrados, could be stepped off, the ribs being separated by felt paper or other suitable separators."

The theory of the action of mud (the foundation) and the theory of the action of setting concrete are of far more importance to designers than the theory of the action of some hypothetical substance that is perfectly elastic and ideal and unattainable rigidity of abutments.

ARCH FAILS BECAUSE OF ERRORS IN DESIGN

The Tacoma arch, already referred to, is described in *Engineering News-Record,* Feb. 22, 1923, p. 355. This was a basket handle or semi-elliptic arch of a span of 87′-5″, built in three bands, and having a large skew. Each band was a skew arch. The arch did what a casual inspection of its design would lead one to anticipate: it failed. The failure was by a horizontal shear crack just where a basket handle arch is the weakest in shear and bending, namely, in the sharp curve of the haunch a little above the springing line. In every vital feature the design of this arch was bad. The abutments were hollow, thus diminishing their stability. The arch curve was a semi-ellipse, thus necessitating active horizontal pressure of the earth fill for stability, a thing that exists only in standard works. The arch had a large skew, 45 degrees, the abutments being located practically outside of a line through the center of figure normal to the axis of the barrel of the arch. The offset of the abutments is still further accentuated by the arch being divided into three bands.

FAILURE OF SERIES OF ARCHES

One of the most extensive of arch failures is described in *Engineering News,* May 13, 1909. This was a series of arches across the Illinois River at Peoria, Illinois. Four Melan reinforced concrete arches of 125 ft. span completely collapsed and another span of 110 ft. sagged.

It was claimed that insecure foundations were the cause of this failure, and it is no doubt true that the foundations were not what they ought to have

been. However, there are other features about the design and construction that point to other contributory causes.

The piers of this bridge rested on piles, another bridge close by stood for 30 years with the piers simply resting on timber cribbing on the bottom of the river.

ARCH WITH HIGH ABUTMENT SAGS—HORIZONTAL PRESSURE ABSENT

The span which sagged was supported at one end by a large pier which also carried one end of a bascule span. The other end was carried by a shore abutment. Both of these were square with the axis of the bridge. The piers at both ends of the bascule span (which was span No. 2) remained firm, for the bascule could be operated after the failure. This would appear to uphold the character of construction in the foundations, if their design had been ample. The shore abutment appears to have been of the ordinary standard type of high abutment depending to a large extent on horizontal pressure of the earth backing to take the arch thrust. This would completely account for the sagging of this arch. It is said of the abutment at the other shore that the fill back of it for several feet of depth was manure and other light materials and then rock and earth up to grade. If any horizontal pressure whatever was assumed to be exerted by this backing, that would aid in supporting the arch, a serious blunder was made.

SKEW ARCHES COLLAPSE—DANGER OF SKEW PIERS

The three arches which collapsed were supported on skew piers and abutments, the skew being very large. They were supported on falsework when the collapse occurred, but had been free of such support previously, being supported thus when it appeared evident that failure was imminent. In my opinion the skew piers of this bridge were a large factor in causing the collapse. Photographs seem to show that the piers were revolved in the collapse. Weak foundations would not account for the collapse of arches whose weight is carried by falsework. But if the arches were previously highly stressed by such eccentric stresses as those of a monolithic arch of heavy skew, and if the piers were subject to heavy twisting forces, it would be in reason to expect that the arches would break up under these internal stresses and collapse as they did. Absence of lateral stability in the falsework would make it incapable of supporting a load which had the very heavy internal lateral stresses that must exist in a monolithic skew arch.

TOO MUCH THEORY RESULTS IN COLLAPSE

Two notable cases of failures of arches both of which occurred in Germany, illustrate what may happen when ultra theoretical ideas are carried

to their logical conclusion. These arches were most carefully made to fit the theory. Designed as elastic arches the designer provided cast hinges at the ends and at the crown. The arches promptly collapsed with little or no load on the arch ribs except their own weight. One of these failures is described in *Engineering News-Record,* Jan. 4, 1923, page 35. The other is described in *Engineering News,* Oct. 27, 1904, page 374. It is said of this one that the failure is "attributed by a technical expert employed to investigate it, to the use of an improper lubricant on the faces of the hinge joints." Wrong kind of grease! Two arches of 144-ft. span dropped. If they had been in use and loaded with people, a hundred might have lost their lives, and we are asked to believe that proper lubrication would have saved this structure! If the designer had used more horse sense and less alleged theory and had anchored the ends into the abutment and continued the rods through the crown and poured concrete where these expensive cast shoes were used, the structures would doubtless be standing today.

CHAPTER III

COLUMNS

Part I, written in 1911.

Column failures are numerous and are the result of several causes. Some of the chief causes can be laid at the door of engineering books and afford other examples of too much mathematics and too little common sense.

TENSILE STRENGTH NEEDED IN COLUMNS

A column to be strong must be made of a material having good tensile strength. Or if the material used has not much tensile strength, it must either be reinforced for tension or given a very low unit stress in the design.

TWO METHODS OF FAILURE OF COLUMNS

A column may fail in two distinct ways, namely, (1) as a bow or spring altogether independent of the ultimate compressive strength or the elastic limit of the metal, (2) as a member in compression, crushing the fibers. These simple and easily demonstrated facts are practically totally absent in engineering books dealing with the subject of columns.

Instead of recognizing the two distinct phases in the strength of a column, the two are combined in the Gordon-Rankine formula, for all lengths of columns, and absurd results are attained. It is true that there is a range (in columns of intermediate lengths) where there is a combination of the effects of spring and crushing of metal. There is also a range (in short columns) where a column could not fail by springing or bowing but only by crushing the metal; as well as a range (in slender columns) where crushing of the metal cannot take place until the column has failed by springing. Failure to recognize these distinct phases of the strength of columns and to separate and differentiate them has led to many gross errors and many failures.

As I have pointed out in *Railway Age Gazette,* July 2, 1909, and more fully in my book "Steel Designing," the Euler load is the absolute maximum load that any column can sustain, irrespective of the ultimate strength or elastic limit of the steel, in spite of the fact that the Gordon-Rankine formula shows for slender columns ultimate strengths several times the Euler load. Books say of the Euler load that "under this load the column just begins to deflect, and will under a constant load retain any deflection which may be given to it, within the elastic limit of the material." The elastic limit does not enter in the formula or its derivation, and there is no proof of any such thing in the common derivation of the Euler formula. Such statements as the one

quoted are misleading, as they give designers the notion that there is still some reserve strength in a column after the Euler load is reached.

The Gordon-Rankine formula has no application whatever to slender columns, and on the other hand the Euler formula has no practical application to short columns. These facts are of more real value to a designing engineer than pages or volumes of intricate mathematical formulas. If they are known to book writers, they have been carefully suppressed. Hand books give the supposed ultimate strength of a pin-ended column in medium steel, whose ratio of slenderness is 240 as nearly 12,000 lbs. per sq. in. by the Gordon-Rankine formula, whereas the absolute ultimate strength of such a column, even if it were made of steel having an elastic limit of 200,000 lbs. per sq. in. or more, is actually only 5,000 lbs. per sq. in. They give the ultimate strength of columns whose ratio is 40 at about 47,000 lbs. per sq. in. whereas such columns under test will not show much more than two-thirds of this ultimate strength.

STRAIGHT-LINE FORMULA PREFERABLE

A straight-line formula for safe loads in columns is the most reliable for several reasons. First, it agrees more closely with experiments than any other. Second, the values lie wholly within the Euler curve. Third, it shows low values for slender columns and thus discourages their use. Fourth, it agrees closely with the theoretical strength of columns on the assumption of proportional shop imperfections. The latter is shown by the paper in *Railway Age Gazette* and in *Steel Designing.*

COMPARISON BETWEEN STANDARDS OF CONCRETE AND CAST IRON COLUMNS

The other basic and suppressed fact referred to in the opening of this chapter is that a column, to be strong, must be of material having good tensile strength. This is so rare a piece of information that when some years ago in a meeting of engineers a well-known investigator stated that high tensile strength in the concrete of a reinforced concrete column increased the compressive strength of the column, one of the principal engineering papers of the country made a special news item of it. I have been emphasizing this fact in my writing since 1907, when I pointed out, in an article in *Concrete Engineering,* the fact that cast iron columns, made of a material having an ultimate crushing strength of about 100,000 lbs. per sq. in., are designed (when properly designed) by a formula the base unit of which is 7,600 lbs. Only 7.6 per cent of the ultimate strength—and this because of the weakness of cast iron in toughness or tension. I also emphasized the blatant error of designing so-called reinforced concrete columns of practically plain concrete, on a formula the base unit of which is 750 lbs. A material one-fiftieth as

strong in compression and one-hundredth as strong in tension with a supposed "safe" unit one-tenth as great!

SLENDERNESS OF COLUMNS NOT UNDERSTOOD

There have been many failures of slender columns. Some of them have been deliberate designs by specialists not aware of their weakness by reason of the misleading character of the information imparted through books. This slenderness is sometimes in the column as a whole and sometimes the result of inadequate means of uniting the component parts of the column to make an integral column of the same.

An experienced designer, proportioning a column by the Gordon-Rankine formula, the ratio of slenderness of the column being about 240, could not understand why such columns showed weakness and why the ultimate strength was not about 20,000 lbs. per sq. in., as the formula showed. I had difficulty in convincing him that the ultimate strength of his column was about one-fourth of that which he assumed, not only because it was too slender to be in the range of the Gordon-Rankine formula, but also because it was not fixed ended but more nearly pin ended. The end connection was to gusset plates in the plane of the web of the light channels of which the compression member was made. This is practically a pin-ended connection, for such gusset plates cannot maintain fixedness of the axis of the member at the ends.

Another feature of the column just referred to was the use of batten plates with the idea that these shortened the unsupported length of the individual channels; as though one weak member could support another equally weak member by being connected thereto by a batten plate which would allow both to deflect sidewise without interference. Here is another exceedingly vital point of design upon which books are silent. A compression member must be capable of taking transverse shear. It is an elementary engineering principle that a rectangular system, such as the battens and channels, cannot take shear. Lattice bars and the channels form a triangular system which can act as a truss to carry the transverse shear of a column.

SLENDER COLUMNS FAIL, HAMBURG GAS HOLDER

In *Engineering News*, July 6, 1911, there is a description of a gasholder post which failed in Hamburg, Germany. This post was made of two little 5-in. channels and a few pairs of little tie plates that could be carried in a man's coat pocket, and it was expected to carry a load of 133,000 lbs. A leading European authority who investigated the wreck and the design reported that "the use of tie-plated columns, when the section is assumed to be integral, may lead to constructions which do not afford adequate security under loading of unusual character."

Fig. 1 shows a sketch of this column and how and why it could bow under

endwise compression. In a letter in *Engineering News,* July 27, 1911, I pointed out the fact that the probable ultimate strength of the column, worked out theoretically, is just about the amount of the load under which it failed—the load that was supposed to be the safe load. But this ultimate load, as I worked it out, was not figured on the basis of the standard book method of designing posts.

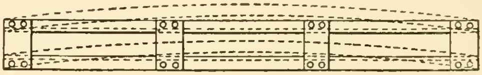


Fig. 1.

EULER FORMULA INAPPLICABLE TO SHORT COLUMNS

I pointed out in this letter, the fallacy of applying Euler's formula to short columns as the designing engineers had done; for they supposed the pair of channels would be united as an integral column by the tie plates, thus making the column a short one. I also pointed out the error in depending on tie plates to unite the parts of a column. The Euler load gives the ultimate load of a column—the load which could not possibly be exceeded by any column no matter how great the compressive strength of the steel may be; but in short columns, that is columns having a ratio of slenderness of 100 or less, structural steel will fail in compression before the springing or bowing action can come into play. Tie plates do not hinder the bowing action of a column such as this one except in an inconsiderable degree. Because of the absence of lattice these two channels can bow together, and the ratio of slenderness of the column is the full length of the column divided by the least radius of gyration of a single channel. In this post that ratio was 180.

TIE PLATES NOT A SUBSTITUTE FOR LATTICE

My contention in the letter above referred to was disputed by a German engineer in a letter published in *Engineering News,* Sept. 28, 1911, and this was answered by me in the same issue. The claim is there made that because the tie plates would have to take the shape of oblique parallelograms by the bowing of the column as shown in Fig. 1, these tie plates would resist such bowing. The argument of my reply will be repeated here.

These small two-rivet plates can scarcely add any rigidity to a column carrying more than 100,000 lbs., except in the immediate vicinity of the plate. Of course in a greatly exaggerated sketch the parallelogram formed by the four rivets of the tie plates would appear to be greatly distorted. The same might be argued if only one rivet connected the tie plate and channel, since friction would resist rotation. Rivets are subject to slip, sometimes under

small stress, and an exceedingly minute slip would allow all the rotation necessary for the column to assume the dotted position of Fig. 1, for a slender column has reached the point of ultimate failure at the first measureable deflection, if it be originally straight.

A slender column whose parts are perfectly straight may reach its ultimate load when it has deflected $\frac{1}{16}$ in. or less, whereas a similar column with an original bow of ⅛ in. will stand an addditional deflection of ⅛ in. before it fails, both columns failing at the same load. These are facts very easy to demonstrate mathematically on the theory of flexure but very difficult to find in literature on the subject. It is such facts as these that ought to be written into the literature of engineering to displace a lot of mathematical nonsense in the way of complex and meaningless column formulas based on impossible assumptions, which totally ignore the practical work of manufacturing a column. Such facts as these would go a long ways toward intelligent design of columns. Such emphasis of the importance and magnitude of slight deflection or bowing in a column has more weight than an abstract dissertation on the impossibility of a rectangle assuming the shape of an oblique parallelogram.

There is a great difference between holding 40 ins. of a column straight with a leverage of 3 ins. (the distance assumed between two rivets of a tie plate) and holding that length of a column straight by a triangular system of lattice. In the column under consideration, in 40 ins. of length (the space between tie plates), one tie plate has the work to perform that in a latticed column would be done by 10 or 15 lattice bars. All of these lattice bars would be acting to resist the bowing of the column, and all of the 20 or 30 rivets aid in relieving the individual channels of the bending which they would have to take in addition to direct stress in the tie plated column.

TIE PLATES MERELY JOIN TWO SLENDER COLUMNS

The failure of a sprinkler tank support is described in *Engineering Record*, Aug. 13, 1910. On four columns consisting each of 4—4″x3″x9.3-lb. Tees held at intervals of 21 in. by 6″x3″x⅜″ tie plates a load of 22,000 gallons of water and two tanks was carried. The columns were 13′-9″ high. The ratio of slenderness is nearly 200, and this load is not far from the ultimate load of the columns. Failure is of course a natural consequence of such construction.

THE RODDED COLUMN

So-called reinforced concrete columns, columns of plain concrete with slender rods embedded in them, are a prolific source of failures; and the marvel of it all is that with the large number of these failures, and the great loss of life and property they have occasioned, the eyes of engineers have not been opened to their absolute untrustworthiness—this in spite of the fact that

laboratory tests point to the same conclusion. Designers go merrily on proportioning and building columns of this kind and discussing the loads they can sustain with an assumption of accuracy that is about as far from truth or science as a Hindoo's incantations over a broken bone are from the science of surgery.

ERROR TO CALL SQUARE TIES HOOPS

If these designers use a square or round shaped wire around their slender rods, spaced a foot or more apart, they call it a hooped column.

This subject is more fully referred to in my paper read before the Am. Soc. C. E. in 1910, on Some Mooted Questions in Reinforced Concrete Design and the discussion which follows.

Since that discussion there has been another failure, another investigation, another batch of conclusions that tell nothing that anyone could not have ascertained with a little inquiry and observation. These reports could be gotten up in blank form and filled in, if frequency of the wrecks should demand it, leaving a place to fill in "block of wood," or "shaving," or "sawdust," and the number of the column in which they were found; also a blank could be left for the location where a bad batch of concrete was found, as well as for the name of the dead workman who pulled out the props before the exact time was up. A few other standard findings could be incorporated in the form, together with a line stating that the design was found faultless.

If a single rivet were omitted in a steel frame building, and the entire building were wrecked on account of this omission, it would be nearly on a par with the supposed reason for these great reinforced concrete building failures, so slender is the hair upon which the safety of these buildings depends. It ought not to be in the range of possibility that a little bad concrete, a little carelessness in cleaning out forms, a little laxness of inspection could result in an awful wreck. It would not be possible if the columns of a building were properly designed.

The recent wreck referred to is that of the Henke Building in Cleveland, Ohio. This wreck is described in *Engineering News*, Dec. 8, 1910. In *Engineering News,* Jan. 26, 1911, appears the report of a commission appointed to investigate this collapse, also a letter on the subject written by me a few days after the published report of the failure. That letter is quoted in full below:

"The latest wreck in reinforced concrete described and illustrated in your issue of Dec. 8, 1910, page 636, is just another finger of scorn pointed at the engineering profession.

"In spite of what the findings may be of the men who are investigating this wreck there is enough in the photographs of the building to condemn its design and place it in the large class with all of the other great wrecks of reinforced concrete structures. They may find a shaving or a wooden block

in the wreck of one of the columns or build a worthless theory on a splinter, as has been done before, but the glaring truth stands out that plain concrete shafts are unfit to support heavy loads, and slender rods in these shafts do not reinforce them; also this harvest of death emphasizes the absolute need of a unifying element in all reinforced concrete structures.

"The pictures of this wreck show two features of its design that condemn it, and yet one could read engineering literature concerning reinforced concrete for days without ever finding a hint of the danger of these features; one can also find many examples of these erroneous features in the work of experts of national prominence. Here is where the profession is to blame for these wrecks.

"In *Engineering News,* in 1906, the writer pointed out the error in depending upon longitudinal rods in a concrete column. He has at various times condemned such design publicly in the most emphatic manner possible. A large reservoir roof in Madrid, a hotel in California, a building in Rochester, another in Philadelphia—all of these, the greatest wrecks recorded, had longitudinal rods in the columns. They could not possibly have failed as they did, if they had had tough steel columns or properly hooped reinforced concrete columns.

"I have repeatedly flaunted these facts in the face of the engineering profession and challenged contradiction. In my paper 'Some Mooted Questions in Reinforced Concrete Design' read last March before the American Society of Civil Engineers and very widely discussed, I made very prominent a severe criticism of this flimsy method of design. Only one critic came forth with anything resembling an argument, and he, by averaging an indiscriminate lot of hooped and other columns with steel in them, tried to satisfy himself that nursery columns are greatly strengthened by slender longtitudinal rods. By cutting out four of these nursery tests in one group the averages for that group would tell exactly the opposite tale. This is supposed to be careful investigation! Some of the tests with no steel whatever were 25 and 50 per cent stronger than others with steel rods in them. Astounding as it may seem the very pamphlet that shows these results, written by one of the best known authorities in this country, *recommends the addition of 17 to 20 per cent to the strength of plain concrete columns for each per cent of slender steel rods added!* Nearly all books on the subject are guilty of the same gross errors. It is time for plain speaking in this matter. Why do not these authorities come out and demonstrate their position with something better than a lot of averages that mask more than they reveal? Or else why do they not acknowledge their error?

"The nursery columns, loaded perfectly central, tell only a part of the weakness of these columns in a monolithic building. The least movement or settlement in such a structure, with no articulation and no toughness, puts excessive strains on the weakest part of the frame. In this construction it

is the columns, that are designed as mere props with no ability to resist bending. Cracks in these mean spalling and stripping of the concrete and disaster.

"In one wreck the entire reinforced concrete roof of a large building collapsed, because the concrete had been frozen and was not set. The top floor and the columns below it were not hurt. The columns were octagonal and were hooped. Their toughness saved the building. All this despite the fact that under test it developed later that the floors were not capable of carrying their supposed safe load.

"The unifying element, referred to in the first part of this letter as being absent in the Cleveland building, is continuous rods through the columns to tie the beams together. In a steel building this is supplied by the rivets. In many reinforced concrete buildings it is lacking. There is no sign of any such design in the photographs of this building. There is no doubt whatever that tenacity is the one thing that a reinforced concrete structure needs both in the whole and in all of its members, particularly the columns. A column that can bulge out or be spalled off by the mere overcoming of the weak and uncertain tenacity of plain concrete is an unsafe column for heavy loads. Highly stressed slender rods aid in this bulging tendency and hence menace the strength of the column they are supposed to reinforce.

"It is a shame and a burning disgrace to the engineering profession that no more is learned from investigations of disasters such as this than that practical construction is not absolutely perfect, materials are not absolutely clean; such reports could be written thousands of miles away from the wrecks. The tremendously important lesson is ignored that these columns are absolutely untrustworthy.

"If a man should aim a revolver point blank at another's heart, and wound him instead through the lung, and if the other man should fall dead of heart failure at hearing the report of the shot, physicians might demonstrate that death was due to fright at hearing the noise and that a cannon cracker might have produced the same effect, and they might clear the murderer by this technicality. If it is found that there was a bad batch of concrete somewhere that set off this wreck, it would supply a technicality such as the other, but it will not clear the engineering profession of wilful ignorance.

It is the profession, as represented by its leaders in framing literature and its foremost members in the reinforced concrete field, that is responsible for this wreck and not the individual designer who may have done his utmost in following standard, though abominable practice."

Part II. This was written in 1923 as a paper on Reinforced Concrete Columns for the Institution of Structural Engineers of London, England. It has since that time been under consideration by the publication committee of the Institution.

The columns of a building are of greater importance than any other parts of the construction. At first blush it may strike one that all parts of a building are equally important; but there is a double or in fact a triple duty on the columns of a building, and hence the stability and integrity of a structure depends in a larger degree on the columns than on any other elements of a structure.

The three functions are these: First, the columns must take their own direct loads. Second, they must take, at least in a large measure, the swaying forces in a building, particularly in buildings where there is no diagonal bracing between columns. The third function of columns is a conditional one which they may be called upon to perform in case of a local failure either in columns or beams due to any cause.

It is no doubt conceded by all that the first two functions of columns, as stated in the preceding paragraph, are legitimate and should be provided for in every structure, also that the type of column should be such that the provision is sure in any design. It is easy to argue that no engineer is expected to design a building proof against the effect of local failure due to any cause. Laying aside argument of this point, for the time, it is desired here to point out the facts showing this phase of the importance of columns in a structure.

If a beam or girder in a building should fail, there will generally be added stress or shock on the columns. This may be due to the beam or slab swinging down and striking the column; it may be due to cantilever loads of the stumps of beams attached to the columns, outside columns being particularly susceptible to this effect; it may be due to the removal of lateral support at a floor. If a column of a building should fail, the effect on the surrounding columns is at once evident. The load which that column carried is immediately thrown on the other columns, and the cantilever effect of the removal of the broken column may be many times that of a direct load of the same amount added to the standing columns.

It is by no means a theoretical matter that the columns of a building may act in a "row-of-blocks" manner if one of these columns should fail. Numerous failures, where all or nearly all of a building has collapsed, have been traced to the exceptional weakness of one column.

If the proposition is made that columns should be designed of a capacity or type to guard against the effects of a collapse, if one column or beam should fail, it may be asked, would not this be a waste of material, and would not columns have to be designed to carry more than their legitimate load? This can be answered in two ways: first, by the history of building failures, and second, by the facts of the case. But first the fault in standard reinforced concrete design of columns will be named.

Reinforced concrete columns of round or of octagonal shape having a set of longitudinal rods arranged in a circle and having round hoops or coils with

a pitch or spacing of about two inches are a type of column to which no exception can be taken. They are both strong and tough.

Columns made of square or oblong section with four, six, or more upright rods, and with square-shaped or oblong shaped ties of small rods or wires spaced about 12″ apart, are a type of column to which exception is taken. They are neither strong nor tough, and they have an inherent weakness that has already been exhibited in a large number of very bad failures.

It is this lack of toughness to which exception is taken. It marks the difference between the rodded column (described in the foregoing paragraph) and the hooped column as well as the difference between the rodded column and the structural steel column.

I know of no failures of beam and column type structures where the columns were either of structural steel or of the hooped reinforced concrete type. A score or more of very bad failures could be named where the columns were of the rodded type. Some of these have been of well seasoned concrete, some of structures nearly ready for use. Universally the columns break up in short pieces, and the alleged reinforcing rods bend in all sorts of curves. They are not tough and therefore have no reserve strength. This is the argument based on the history of failures.

The argument of facts is this. Building columns are seldom loaded to their legitimate capacity. That is, it is very seldom that all floors and the roof of a building are loaded at once. In any properly designed building there will therefore be a reserve strength in the columns under any ordinary condition of loading. And this reserve strength is not a waste of material. In spite of this supposed reserve strength many reinforced concrete buildings have collapsed in ruins, and in none of these cases has the cause been a general overloading of the columns. These facts are of tremendous importance, and they demand some explanation.

It is inconceivable that the fault in these failures can be attributed to the beams and girders, for if the origin of the failure, that is, the tripping cause, be a local weakness in beam or girder, there is no reason why the thing should be communicated from beam to beam. On the other hand, if the origin be in a column, there is every reason on earth to anticipate that if the other columns are only a little better than the one which lets go, all or nearly all of them will go down in the wreck. This can only be true of columns that lack in toughness and reserve strength and are therefore of an unfit type for the purpose.

It would consume too much time and space in the journal of this Institution to repeat the details of the large number of failures of reinforced concrete structures to which reference has already been made in this paper. A list of 29 of these will be found named and, in a general way, described in *Concrete* of Detroit, February, 1921. The greater number of these have been failures due to the weakness of columns. All of the columns are of the

rodded, so-called reinforced concrete, type. Not one of these failures could possibly have happened in the way they did, if the columns had been hooped. Failures of hooped column installations are conspicuous by their total absence. I have repeatedly made the foregoing statements publicly, and they have yet to be challenged or contradicted.

With the foregoing introduction my readers and hearers will understand that this paper is written for the purpose of denouncing the rodded column as an unfit element in a structural design, but before entering into that denunciation and the ground therefor I shall first show that it is standard and accepted practice to use such columns.

An epitome of practice will be given as the same is revealed in engineering works and in reports on standard practice.

In "Principles of Reinforced Concrete Construction" by Turneaure and Maurer, 1907, on page 106 one of the ways of reinforcing columns is stated to be "by means of longitudinal rods extending the full length of the column." On page 108 percentages of increase in strength for this type of column are worked out (for different concrete moduli) up to 39 per cent for each one per cent of steel rods. On pages 255 and 256 examples of rodded columns are given. These authors, however, recognize the fact that hooped columns are tougher than rodded columns.

In "Concrete, Plain and Reinforced" by Taylor and Thompson, 1916, page 633 gives examples of rodded columns. Ties are spaced about 12″ apart in one of these. On page 634 the rule given for spacing the ties is this, "The spacing of these ties should not exceed 18″ or the smallest diameter of the column." Some of the rodded columns shown in this book are square with eight longitudinal rods, four of which are in the middle of the side of the column. The ties are in the shape of a square, so that they have little use, if any, as ties for these four rods.

Buel and Hill, 1906, page 217, give examples of rodded columns, ties spaced nearly a foot apart, also examples on page 212.

Hool and Johnson in "Concrete Engineers' Handbook," 1918, give examples on pages 374 and 407. The latter shows a 20-in. square column with square ties and twelve upright rods arranged in a square. These authors use the Joint Committee Report of 1916 as a standard but improve on that Report by counting out 1½″ of concrete in the effective area.

I have never been able to find a single standard author who does not accept the rodded column as a proper type of reinforced concrete column or who questions its strength or suitability for buildings. I have found many practical designers who condemn and avoid it, some of them members of committees which have reported the rodded column as standard and safe. Unfortunately, a large part of the designing is done by men who blindly follow standard authors and makers of reports and codes.

The peregrinations of the Joint Committee on Concrete and Reinforced

Concrete, on the subject of columns are most interesting. (This is a committee of representatives from all of the national engineering societies, interested in structures and materials, in America.)

The 1909 Report allowed rodded columns and prescribed a unit stress of 450 lb. per sq. in. on the core, after 1½″ of a concrete shell was counted out. This was considered as a protective covering and not included in the effective section. (A column 10-in. square in 1909 was good for 22,050 lb. plus the steel value.)

In the 1912 Report the unit is still 450 lb. and a 2-in. shell is counted out. (The 10-in. column was then good for 16,200 lb. on the concrete.)

The 1916 Report allowed 450 lb. per sq. in. and did not count any of the concrete out as protective covering. (Ten-inch column good for 45,000 lb., more than twice as much as in 1909 and nearly three times as much as in 1912.)

The 1920 Report allows 400 lb. as a unit on the full area. (Ten-inch column good for 40,000 lb., a small set-back.)

So much for the alleged safe capacity of rodded columns as years roll by and as different individuals interpret exactly the same data and some of the same individuals sign their names on the dotted line.

Other allowances and provisions on columns by these several Reports are still more startling. The 1909 and 1912 Reports allow plain concrete columns up to 12 diameters. Prof. A. N. Talbot in the discussion of my paper read before the American Concrete Institute in 1915 (which discussion was suppressed by the officers of the Institute) said, "The members of the Joint Committee think that the report does not permit plain concrete columns in reinforced concrete work. It may have made the statement a little awkwardly, but that is the fault of the wording."

Here is the wording of these reports.

"Axial Compression."

"For concentric compression on a plain concrete column or pier, the length of which does not exceed 12 diameters, 22.50% of the compressive strength or 450 lb. per sq. in. on 2000 lb. concrete, may be allowed." (See Trans. Am. Soc. C. E. Vol. LXVI, 1910, p. 452, and Vol. LXXVII, 1914, p. 424.)

Awkward wording does not excuse this. The ten commandments are not in any plainer or more unequivocal language than this. It is significant that the 1916 Report did not attempt to revise this wording but simply eliminated the paragraph altogether.

In another respect the pronouncements of the Joint Committee on the Columns are of interest. The 1912 Report calls a column a compression member "of which the ratio of unsupported length to least width exceeds six." In the 1916 Report the limiting ratio is 4, and in the 1920 report it is 3. Standing by itself this looks like improvement in standards, but it really has little to do with actual design. In 1915 a very elaborate series of tests was

made public at the American Concrete Institute. These tests were on piers about five times as high as their diameter but they were called columns. The Joint Committee's definition classed them, at that time, as piers, and justification of column design was depended upon on the basis of these alleged column tests. This may be the reason for the change in the definition of a column, as, otherwise, there was no experimental basis for the standard allowances on columns. Of this, and these tests more will be said later.

In still another respect the Joint Committee reports of the several periods are at variance, though a distinct progression is evident toward safe design. In the 1909 and 1912 reports it is stated that "bars composing longitudinal reinforcement shall have sufficient lateral support to be securely held in place until the concrete has set." If this means anything, it means that the function of the ties is merely to hold the longitudinal rods in place during construction, and any kind of broom wire would be enough to hold the rods securely in place until the concrete has set. In fact buildings have been constructed in which the columns have no ties; it cannot be said that these violate the spirit of these reports, for the rods were held in place until the concrete had set. There is not an engineer living who would approve this type of column (the type of the 1909 and 1912 reports) at the present day. Those who protested against it seven years ago and more, when it was an American standard, can be counted on the fingers of a maimed hand.

The 1916 Joint Committee Report still had part of the old shell hanging to it in the form of a provision that longitudinal bars must have sufficient lateral support to be held in place until the concrete has set. In another part of the Report, however, ties are specified to be ¼″ in diameter and spaced at least 12″ apart or 16 diameters of the rods.

The 1920 Joint Committee Report limits the spacing of ties in rodded columns to a minimum of 8″. Another step or two and a safe column would result, provided it were possible to induce designers either to avoid this type of column entirely where loads are not balanced or to take into account all bending stresses due to unbalanced or eccentric loading.

The American Concrete Institute standards for columns have in a general way paralleled those of the Joint Committee, though ties have been definitely called for and their spacing required to be at least 12″ or the diameter of the column. From 1916 on, the spacing of ties has been limited to 15 diameters of the rods.

I was on the Joint Committee in 1916 and on the committee which formulated the "Standards" in 1916 in the American Concrete Institute, but in both cases I publicly dissented from every word of these reports concerning rodded columns. No other member of either committee joined me in this, though one member informed me four years later that he would have signed my dissenting note but for the fact that he had previously promised to vote with the majority.

Continental and English practice as regards concrete columns reinforced with longitudinal rods and ties seems to be better than American in some respects. The regulations of the Hungarian Engineers and Architects of 1911, as given in Proc., Nat. Asso. of Cement Users (United States), Vol. VII, allow 512 lb. per sq. in. on the concrete, which is more than the Joint Committee; and they require ties spaced the column diameter or not more than 12″ apart, which is better than the Joint Committee Report of that period.

Faber and Bowie in their book, "Reinforced Concrete Design," 1919, Vol. I, p. 101, give description and a formula for rodded columns, spacing the ties up to *d*, the diameter of the column. An allowance of 560 lb. on 2000 lb. concrete is recommended which is stated to be in accordance with the French rules. These rules, the authors say, are the best, and the Royal Institute of British Architects copied the same in their report of 1911. This report is given by the authors. The London County Regulations are also given by Faber and Bowie in this book. It is scarcely necessary to go into detail in pointing out just what allowance is made by these several regulations. It is complex, unnecessarily so. The strength of columns is made to depend on variables that could not possibly have any basis in tests for the simple reason, as will be pointed out later, that many alleged reinforced concrete columns have not stood as great loads as some plain concrete columns. More will be said later on variation of unit stress depending on the spacing of ties as allowed in these regulations.

The Reinforced Concrete Regulations of the London County Council restrict the spacing of ties to six-tenths of the diameter of the column or to 16 times the diameter of the vertical bar and require even closer spacing near ends of columns. The R. I. B. A. rules seem also to limit the spacing of ties to six-tenths of the diameter of the column.

The French rules as given by Faber and Bowie give formulas for rodded columns with spacing of ties up to the diameter of the column. In this feature the British practice, as exhibited by the London County and the R. I. B. A. standards, is a great improvement over the French as well as the American practice.

In another respect rodded columns as shown by Faber and Bowie in the text of their book and as quoted by them from the R. I. B. A. rules are vestly better in design than in any American standard practice. This concerns the use of cross ties where there are more than four longitudinal rods. It would seem like wasting words to do more than merely mention the need of separate ties through the column for rods in the middle of the sides of columns. The absurdity of trying to tie in the vertical rod A with a straight horizontal rod B, as in Fig. 1, is so patent that it seems superfluous to do more than mention it. And yet this is very common in American practice, practically universal, and it is done in work of the greatest importance and

frequently illustrated in our engineering periodicals and journals of engineering societies of the highest type. Large areas of steel rods lying close to the surface of concrete will be shown with only small rods parallel to the surface masquerading as ties to hold these rods in place. This cannot be too severely

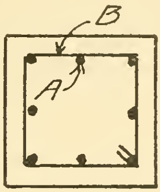


Fig. 1

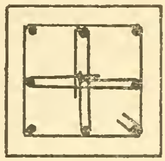


Fig. 2

condemned. I know of no standard in America and no American standard author that require cross ties as shown in Fig. 2 in rodded columns.

The foregoing is sufficient to show the following:

(a) Concrete columns with a slender rod in each corner are universally recognized as reinforced concrete columns.

(b) Ties to hold these rods in place have only in recent years been recognized to have an office in a finished column, in the American standard known as the Joint Committee Report.

(c) Standards in the matter of column design are and have been of a character that suggests the need of an engineering commission whose proceedings are entirely public and open, and in which any participant so desiring will be heard, without resort to muzzle or suppression. This commission should be obligated to answer in full any arguments against any proposed standard of design which they put forth. The present Joint Committee refuses to discuss in any way the *ipse dixit* which they have given out, and all of their deliberations are in secret.

Arguments for the rodded column, so far as I can discern, are four in number. They are as follows:

Argument No. 1. This type of column is allowed by building codes and committee reports. It is standard, why disturb it?

No answer will be attempted to this.

Argument No. 2. Most of the buildings having rodded columns are standing and performing their function. This is an actual argument put to me before the American Concrete Institute Convention in 1915. It was demanded of me that I state what proportion of buildings having rodded columns had failed, the inference being that if this proportion is small, rodded columns are safe. I replied that I was not interested in this proportion but could state that the proportion of buildings having structural steel columns or hooped reinforced concrete columns that have collapsed was absolute zero.

Until any type of design can show this same record it can lay no claim to safety. In Cleveland, Ohio, a large building being finished for opening as a department store a week or so later collapsed in utter ruin. Not a single story length of a single column was left standing upright. This is enough, without mentioning a score or more of other failures of standard rodded column installations, to condemn that type of column forever. In all of these failures the design has been pronounced correct, and the standard of measurement in judging this correctness has been standard works, standard committee reports, standard building codes.

Argument No. 3. This argument is the showing of tests. Tests, of course, are competent evidence. Test and analysis, including in the former experience as well as experiments, are the twin props that support the structure called engineering. But tests must be interpreted rightly, or their value may be negative, that is, less than nothing. In two respects tests on concrete columns have been falsely interpreted. Laboratory tests that do not approximate actual building conditions have only a limited value in determining standards of design. Ideal tests must not be taken at their face value. This has not been observed: standards have been based on ideally conditioned laboratory tests, and when the actual condition of unsymmetrical beam loading is brought to bear on the building columns, failure has been the result in many cases, though columns received only a part of their design load.

The second way in which laboratory tests have been misinterpreted is in the placing of undue importance on average values and ignoring the meaning of low values. When there is a wide range between high and low values, the average does not mean much. The low values should not be ruled out as freaks, but should be accorded a place of importance as possibilities in building construction. When the ideal conditions of the laboratory produce low results, what can be expected on the job? Instead of low results being masked by averages they should be used as the basis of design.

In Trans. Am. Soc. C. E., Vol. LXX, p. 130, 1910, Mr. S. E. Thompson, in arguing for the rodded column, cited the tests made by Prof. A. N. Talbot and reported in Bulletin No. 10, University of Illinois Engineering Experiment Station. By averaging results on plain and on rodded columns he appears to prove that the longitudinal rods add to the ultimate strength of concrete columns. In that set of tests one of the plain concrete columns took an ultimate load nearly 50% more than one of the alleged reinforced concrete columns. Furthermore if two of the rodded column tests had not been made, as well as two of the plain concrete columns, the averages would have shown greater strength for the plain columns. This is proof of two things, namely, upright rods held in place by square shaped ties spaced about a foot apart (the standard type), add no definite value to the strength of a concrete column, and any formula ascribing a value to steel rods in this type of column is of no value whatever.

In the same reference Mr. Thompson averages tests on hooped and rodded and banded columns and compares them with plain concrete columns in an effort to show the benefit of longitudinal rods.

More will be said concerning interpretation of tests and the showing of tests of record. Argument No. 4 will now be touched upon. This is the argument made by several engineers in published utterances, and is in fact the basis of design for rodded columns.

In Trans. Am. Soc. C. E., Vol. LXX, p. 132, Mr. Thompson enunciates the principle. It is the principle that longitudinal rods in a concrete column *must* take stress. The inference is then that they *must* reinforce the column.

Prof. A. N. Talbot in the suppressed discussion already referred to goes to great length to prove that these rods must take compression. He reiterates this several times in controverting my stand against rodded columns. In my reply and closure I say just as vehemently as he that the rods *do take compression.* In fact I have never said otherwise. This is the crux of the whole matter. Concrete, by its very nature, shrinks in setting, and by reason of its virtual shortening it is unable to take its share of the column load. This throws an excessive load on the slender rods, and instead of having a reinforced concrete column we have a reinforced steel column—reinforced with concrete. No analysis is needed to show that four slender upright rods, tied at intervals of a foot with a little rod or wire does not constitute a proper column, even if that disjointed system is embedded in concrete. It is no wonder that tests on rodded columns show many specimens that are weaker than plain concrete columns made of the same grade of concrete. It is no wonder that erratic results are the rule. Analysis would lead one to expect exactly this, and no tests can ever show anything else so long as the properties of concrete are what they are.

The assumption that concrete and steel are in an initial state of no stress is not a vicious one in the case of beams and slabs, where tension in steel rods is the thing to be determined or tested, even if it is not realized; for the very presence of tension in concrete surrounding the steel means that that tension may be useful in supporting loads and relieving steel of tension, and if the concrete cracks, the steel is always present to perform its function. But it is obvious that if the concrete of a column is in tension, this adds to the compression in the upright rods, and load on the column further augments this compression.

Hundreds of rodded columns in buildings and reservoir roofs have failed and dropped their load, and the rods have been found buckled out and curled up, proving that the compression on these rods was excessive.

It is a matter of the simplest kind of analysis that when the longitudinal rods are subject to excessive stress, they will buckle out and spall off the concrete. This at once greatly weakens the column by reason of the crippled rod, the broken concrete, and the eccentric load on the remaining concrete.

In Proc. Am. Soc. C. E., Vol. XLIX, No. 2, Mr. John Tucker states repeatedly that longitudinal rods in a rodded column do not buckle out and spall the overlying concrete. He says that this is a phase of the failure of the testing of this type of column and represents the buckling of the rods after the maximum strength of the column and concrete has been passed and the concrete has been broken allowing the rods to buckle outward. This is a mere dictum on his part, for he sets up no argument whatever to sustain it. He does not deny that rods do buckle out and concrete spalls off and the column then fails. His assertion then really means that he has some occult means of knowing that the concrete in the heart of the column was just ready to give up the ghost in any event.

Pages could be filled with data concerning building failures where the columns broke up in ways that would be utterly impossible, if the columns were tough and strong—if the type of column were proper. In the Edison building, the partial failure of which (after a fire) is described and illustrated in *Engineering News,* Vol. 72, p. 1234, in a large number of cases rods were buckled out of the columns in bow shape, proving not only their excess length but also their excess load. This happened not only in the case of rods in the corner of the column but also in the case of rods in the middle of the side of the column. More remarkable still, rods burst out of columns in rooms where the heat was not sufficient to melt the insulation on electric wiring.

It is true that the rods in the Edison Building Columns did not have ties, but as already stated, they fulfilled the spirit of the Joint Committee Report in force at the time the building was designed, as that report requires only ties enough to hold the rods in place while the concrete hardens.

In the unpublished discussion before the American Concrete Institute in 1915, Prof. A. N. Talbot described some tests made to determine the force necessary to hold slender rods in line while they are under compression. It was found that steel rods 1″ in diameter and 10 feet long subject to a compression of 30,000 lb. required 20 lb. per lineal inch to hold them in line. This information was given as an argument for rodded columns. The weakness of this argument is threefold. First, it makes the column a steel column reinforced with concrete, as tension on the concrete must hold the slender rod in line. Second, the rods may not be straight nor vertical. Third, the shrinkage on the concrete may put the rods in stress that it is impossible to determine.

Turneaure and Maurer, in their book Principles of Reinforced Concrete Construction, p. 158, in commenting on the tests by Talbot in Bulletin No. 10, already referred to, say, "The low value for ultimate strength of the reinforced columns appeared to be due to a lower actual crushing strength of the concrete in these columns than in the plain columns." This looks like an effort to fit tests into a preconceived theory. The concrete was all of the same character. If steel reinforces a column when used in the form of slen-

der rods, not confined against buckling, it should reinforce in a hundred cases out of a hundred.

Reinforced concrete standards and standard works almost universally recommend the addition of 15 per cent, to the strength of a rodded column for each one per cent of longitudinal steel. The only way that this can ever be made to *appear* to be true is by "allocating" to the concrete of a test an arbitrary value to fit the formula, as was evidently done by the writer of the quotation of the preceding paragraph. Here are some sample showings of tests.

Of the tests made by A. N. Talbot and others and reported to the American Concrete Institute in 1915 the following results are reported.

The tests are on piers with large heads and therefore there was little or no column action possible. These round piers were made with the greatest care; and if there is any truth whatever in the standard rules for proportioning circular shafts having steel rods and ties or hooping embedded in them, it should be demonstrated by these tests.

(a) The hooped piers having one per cent of longitudinal steel are on the average only 1½ per cent stronger than those having no longitudinal steel instead of 15 per cent, as per standard rules. Standard rules 900 per cent off.

(b) One hooped specimen with no longitudinal steel is stronger than any of those with one per cent longitudinal steel and only exceeded by one of the group with 2 per cent longitudinal steel.

(c) One hooped specimen with 6 per cent longitudinal steel is only 10 per cent stronger than a specimen with no longitudinal steel instead of 90 per cent.

(d) Of the rodded piers the group with 2 per cent longitudinal steel is weaker than that with 1 per cent.

There is no doubt whatever that if these had been columns instead of piers the results would have been still more erratic. Is anything more needed to prove that longitudinal steel does not add any definite value to the ultimate strength even of a hooped shaft of concrete? Longitudinal steel rods are useful and necessary in a hooped column, that is, one reinforced with a spiral or close-spaced hooping, but the concrete is the thing that should be depended upon to take the compression. The steel should be considered as a reinforcing cage and the column designed accordingly.

Here are some significant facts on test results on concrete columns, plain, rodded, and hooped.

(1) Rodded columns in tests show little if any strength in excess of the ultimate strength of plain concrete of the same character in a short cylinder.

(2) Many hooped concrete columns have shown ultimate strengths very largely in excess of that of short cylinders of plain concrete.

(3) Columns of all types, whether rodded, hooped or reinforced with a spiral show practically the same unit stress at the point of initial failure.

(4) In the case of rodded columns the point of initial failure is also the point of final failure; for when the column begins to crack or spall, it generally fails completely.

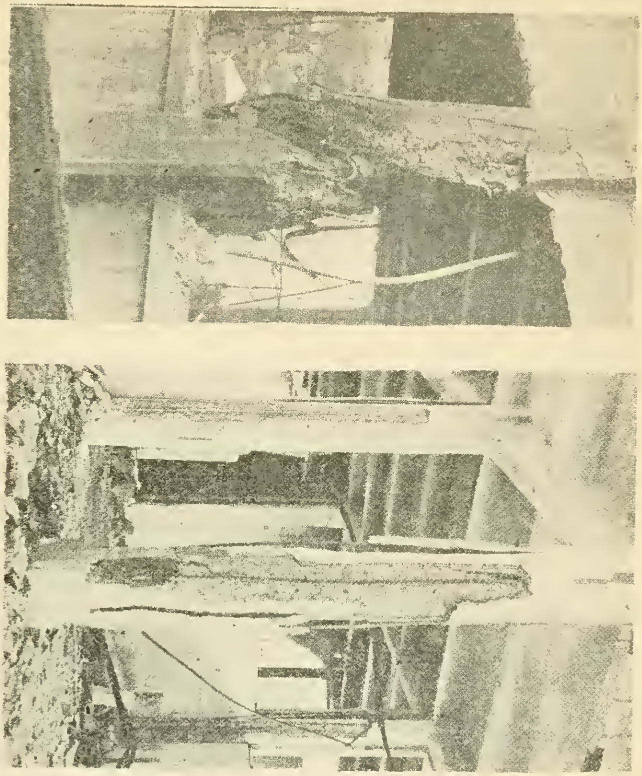
(5) In the case of properly hooped columns the columns will stand a large increase of load after the first sign of failure by cracking and spalling. This increase sometimes amounts to more than the load at first sign of failure, and herein lies the great value of the hooped column as a unit in the design of a building.

In Trans. Am. Soc. C. E., Vol. LXXVIII, 1915, Messrs. C. G. Wrentmore, Hugh Brodie, and C. O. Carey describe a series of tests made on plain and hooped columns. These authors, in comparing results of plain and hooped columns, use the core area for the hooped columns but the full concrete area for the plain columns. In my discussion, in the same volume I point out that if the full area of concrete be used in all cases, the following remarkable facts are revealed. The average load at first sign of failure for the 24 plain columns is 2250 lb. per sq. in., the average load at first sign of failure for the 18 columns having less than one per cent of hooping is 2250 lb. per sq. in.; the average load at first sign of failure for the 28 columns having more than 1 per cent of hooping is 2230 lb. per sq. in.; and the corresponding values of the 10 highest in the foregoing groups are:

Plain columns, 3057 lb. per sq. in. Less than 1% hooping, 2614 lb. per sq. in. More than 1% hooping, 2769 lb. per sq. in.

Other cases could be cited where experimenters and authors have discovered or pointed out that columns of all grades begin to show signs of failure at a unit load on the full area of the concrete which is practically constant for a given grade of concrete. The presence or absence either of vertical steel or steel hooping has little or no effect on the first sign of failure of a column of a given gross area.

In Bulletin No. 20, University of Illinois Engineering Experiment Station, page 29, Prof. Talbot says: "At a load equal to that which would cause failure in a plain concrete column, or a little above, the concrete over the spacing bars begins to scale, and this is soon followed with a scaling and shelling off of the surface of the column over the hoops everywhere." This is said of a series of columns where hooping, without longitudinal rods, added from 31 to 100 per cent to the strength of the concrete at ultimate strength. The ultimate strength, of course, is quite different in a properly reinforced concrete column as compared with one that is not properly reinforced; and the ability to stand eccentric loading is correspondingly enormously greater in the proper column (a hooped one) than in the improper column (a rodded one). This is the secret of safe design that the engineering profession has so far refused to see.



(a) Fig. 3. (b)

How rods "reinforce" a rodded column in a fire.
These columns are standing - not by virtue of any strength contributed by the rods.

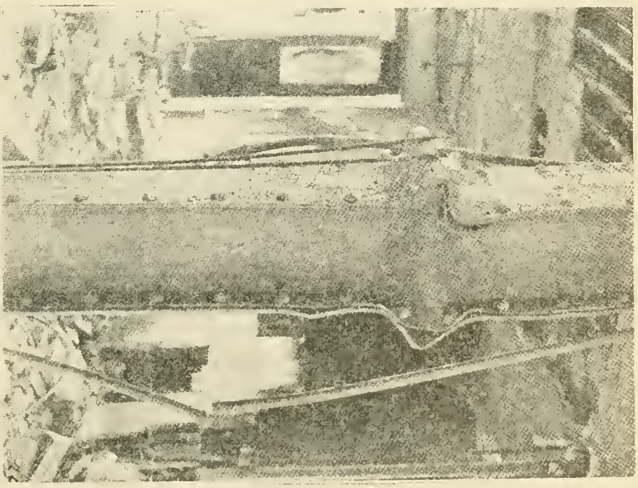


Fig. 4.

The <u>toughness</u> of a steel column may save a structure from collapse even though nearly melted in a fire.

The initial point of failure of a column is the limit of usefulness and should be reckoned as the ultimate strength, for the load that causes signs of failure in a column will doubtless, under repeated application, wear out and ultimately wreck the column. Hence a hooped column has little advantage over a rodded column, so far as unit stress is concerned, that is, the working load of a hooped column should be based on the point of initial failure and not on the ultimate strength as exhibited in a testing machine. But a hooped column has an enormous advantage over a rodded column when safety of a structure is considered, for its surplus strength is a factor of safety and an assurance against general collapse which is lacking in the rodded column of the ordinary American standard.

Rodded columns have a quality in common with cast iron columns, namely brittleness, though the rodded column is even much more brittle than the cast iron column. Columns reinforced with hoops or spirals, closely spaced, and having longitudinal rods as well, have a quality in common with structural steel columns, namely, toughness.

Steel cage building is a name used in America to designate high buildings having a steel frame of columns and beams with all floor and wall loads supported on the same. Many of these are doubtless inadequate in design, but none of them have collapsed in toto. One large building having cast iron columns collapsed into ruins, showing the error of using columns lacking in toughness.

I have seen photographs of steel columns which had been so softened in a fire that they were shortened by several inches, but they were still carrying their load. (See Figs. 3 and 4.)

No structure in reinforced concrete having hooped columns has collapsed. A score or more with rodded columns have collapsed. Always there is found an expert to say that the design was standard and good. Toughness is the saving quality of the hooped column structures, and brittleness is the cause of failure of the rodded column structures.

Laboratory tests, in the nature of things, will not discover the relative toughness of columns, except in a limited degree. This is for the reason that they do not duplicate the conditions found in a building. Extreme care is always taken to avoid, in laboratory tests, the conditions of a building. Exact centering of the load, greased spherical bearings in the heads of the testing machine, careful bedding of the ends—these things contribute to the nicety of test making, to say nothing of the care used in making and placing concrete and keeping the steel in proper position. There are so many ways in which test columns differ from actual building columns that it is a wonder that engineers have not long ago discarded laboratory results and made at least a few column tests where the conditions resemble those found in a building.

Laboratory experimenters have repeatedly taken the stand that the results

of actual failures of buildings have no value whatever as data on which to base conclusions. In other words, actual buildings that collapse give no information of value to designers, because they were not constructed and tested under laboratory regulations. Could anything be more absurd? And yet I can take no other interpretation out of repeated assertions of my critics that my papers contain no data whatever—nothing but my private views: the papers are full of description of failures of buildings where unit stresses in columns are low, and they point out the fact that these failures, in a measure transcending laboratory results, point the way to correct and safe design.

The only test I have been able to find on rodded columns where the columns resembled building construction is described in *Engineering Record,* Sept. 30, 1905. In this test four columns carried two girders, and these girders supported a slab. In the test the columns failed at about 400 lb. per sq. in. on the full section of the columns. The girders, of course, were on one side of the columns, and the load was thus eccentric; also they had a large deflection before the failure took place. These are the very conditions that are to be expected in a building.

I know of a building where a rodded column of thoroughly seasoned concrete failed at 200 lbs. per sq. in. of load. In my list of 29 failures, already referred to, I cite a lot of columns that failed at the ridiculously low load of 150 lb. per sq. in. These and other facts prove conclusively that a reduction in the unit stress allowed on rodded columns will not meet the difficulty. It is the type of column that is at fault, and the logical thing to do is to alter the type or abandon it altogether.

Laboratory tests will not discover the weakness of this type of column: they merely emphasize the unreliability of the type and the totally irregular results to be expected, even under the most perfect manipulation. The building failures tell in unmistakable terms the fact that rodded columns are an improper type for general use, and, if used at all, it must be with restrictions not heretofore a part of any standard. For many years I have been emphasizing the error of this type of column both from the standpoint of analysis and of the showing of real tests, that is, actual buildings. The arguments made in response to my analysis have already been given here. Even these arguments are no longer aired. I have no doubt that if a frank and free discussion of this matter were entered into, it would result in a conclusion being reached, and I have no doubt that that conclusion would embrace a change in the standards of designing reinforced concrete columns.

I have sought a frank and free discussion of a number of features of designing structures, but I have more than once had my entire contribution returned to me with an absolute refusal to publish any part of it, no matter what revision I should make. In 1917 I attempted to discuss a paper on a hollow dam which had failed because of under pressure. The engineer who had reconstructed the dam gave as the cause of failure the foundation. My

contribution was rejected. About the same time I sent a contribution to a discussion on flat slab design which was also rejected. I sent in a contribution to the same society when the last Joint Committee Report was under discussion. They cut out a large part of it. To another engineering society I sent a paper on shear reinforcement. The publication committee acknowledged that there was much in the paper that ought to be made public but refused to accept it because they did not agree with other parts and feared the effect on minds not so hard set as their own. This same society solicited a contribution to the discussion of a paper on two large building failures, and the secretary thanked me when he received it, saying that it would be published. Later the contribution was rejected. I suppose the wise publication committee was afraid of its effect on impressionable minds.

When I was a member of the Joint Committee in 1916, an attempt was made to put a stop to my criticism, inside and outside of the committee, of the previous reports. It was almost demanded in the committee that I accept the majority report. Failing in this a resolution was passed that I go down to Atlantic City to the final meeting and defend myself with data and evidence. I went prepared, and it was only after a fight that I was accorded time to present my arguments. No response was made on the part of my opponents, and the report was adopted with the provisions for columns to which I have already referred.

The facts of the two preceding paragraphs would not be of interest to the profession except that they prove the existence of a clearly defined prejudice against entering into any open and untrammeled discussion of the things that vitally affect safe design of structures. They also illustrate the stagnation, the actual resistance to progress, that affects professions.

I appeared for a day's session before the Joint Committee which made the latest report, in New York, pleading for safe design in columns and beams. I had asked for the privilege of seeing and discussing their report before it was published. This hearing was all they would give me, and the only kind of response my plea elicited was of the nature of heckling. Two members, with the data in their hands, actually denied an assertion of mine concerning tests that one of these men had made. At noon time I looked up the data and found I was entirely right and could have gone farther.

I am not airing personal matters at all. I am on good terms with all of these men. It is simply a type of blindness and prejudgment, and I can see no way to get around it except by persistent reiteration.

In *Engineering News,* Oct. 12, 1911, p. 446, after referring to the fact that the profession was 18 years coming to a realization of the fact that dams should be designed for under pressure, I made the following statement concerning rodded columns: "It takes years and years to pry loose from a body of professional men theories and hypotheses that they have held as standards, and sometimes it takes a large number of awful wrecks. If it takes 18 years

to prove to engineers the danger of concrete shafts with slender rods in them masquerading as reinforced concrete columns, we can expect universal acceptance about 1924, with wrecks every few months in the meantime." The wrecks have gone on apace. Only the large ones are published. The largest one recently was at Salina, Kansas. A large building with rodded columns and long, shallow girders collapsed and had to be rebuilt in another type of construction.

False interpretation of tests, failure to take account of essentials and failure to see analogy or lack of analogy between the laboratory and the structure, these are the things that have produced false standards of design both in beams and in columns.

It is manifest that a column in plain concrete is not a proper structural member in spite of the fact that a plain concrete column will take a large load in a testing machine. It ought to be equally manifest that by putting a slender rod in each corner of a plain concrete column, without ties, the member is improved little if any. Experimenters have been wise enough not to attempt to test columns of this type, though builders have not been so wise. Wherever ties have been used in test columns, the results have clearly shown that they have a significant value. But not until very recent years has this value been recognized, in American standards at least.

It would seem to be an obvious thing that ties should play an important part in stiffening slender steel rods which are tugging at a thin layer of concrete to spall it off, when those upright rods are under heavy compression due to the shrinkage of the concrete and the load on the column. But the ties or hoops or coils have another office to perform that is of equal if not greater importance. This office is that of rendering the concrete itself capable of taking greater compression or of taking a given compression with a greater degree of safety. It is in recognition of this feature of the strength of a column that interpretation of tests has been sadly at fault.

It is recognized that a plain concrete column is weaker than a cylinder of two diameters in height, the standard test specimen, and that this cylinder is weaker than a cube. Some even recognize that a thin disc is stronger than a cube. The fact is that there is a very great difference between the compressive strength of concrete and mortar and stone in thin discs and in cubes, the former having sometimes several times as great an ultimate strength. Lateral confinement is the element that produces this great difference in a property of concrete which in every other way is identical.

Ties in a column, in a measure, convert it into a succession of superimposed blocks with their end planes laterally confined by the ties. Hoops in a column, in a far more marked degree, convert the column into a succession of discs, the distance between the hoops being the thickness of the disc. A reinforcing coil acts in a similar manner.

Considère and others have interpreted the added strength of a hooped

column as dependent solely on the area of the steel hooping, and the formulas show an increase of strength varied with the area of the hooping. They ignore the spacing of the hoops, when it is in fact close spacing of hoops that gives the added strength; for, given a certain spacing in a column, there is absolutely no way by which the column strength could be varied by varying the steel coil area, unless that area were deficient and added area were needed to balance the proportions of the column.

In a paper by John Tucker, Jr., on reinforced concrete columns, read before the American Society of Civil Engineers, Feb. 14, 1923, the author of the paper gives a list of tests on hooped columns (coil reinforcement) and a formula for the increased strength due to increased steel area in the coil. In order to make the tests fit his formula, even approximately, he had to ignore two groups of tests because they showed such high strength, the strength of these columns was so large for the percentage of steel hooping. In the list of tests some columns with ½ of one per cent of hooping were stronger than others with one per cent of hooping. The author of this paper ignored altogether the pitch of the coil: this was not given. I found by looking up the tests that the pitch of the coils in nearly all of the columns given was almost exactly the same, being about an inch. His formula for added strength was therefore of very limited application. It is not enough to say merely that a column is reinforced with hoops or coils. A coil of very wide pitch will not give the same ultimate strength as one with the same volume of steel but having a small pitch.

In British and Continental regulations spacing of ties is recognized as having an influence, the closer the spacing of the ties the greater the allowance on the columns.

While it is true that in general close spacing of hoops or coils will give the greatest ultimate strength to a column, there is a limit beyond which it is uneconomical and even useless to go in search of high strength. I have already said that the shell of a hooped column governs the unit compression that may properly be employed. The shell would spall off before the column approached its ultimate strength or, in some cases, with the excessively high unit stresses recommended in some standards, the shell might spall off at the alleged safe load on the column.

Unit stresses on hooped columns in many standards are too high. The high units are based on the showing of tests at ultimate strength, and they do not take into account the point of incipient failure.

Having expressed disapproval of practically all the standards for the design of reinforced concrete columns in use, both from the standpoint of their basis of derivation and from the results derived, this paper would not be complete without a statement of what I consider the proper basis for deriving formulas and methods of design and what these formulas and methods should be.

In the first place it must be recognized that the engineer's first consideration is safety. Economy comes next, but it should also be realized that the columns are not a large part of the volume of a structure, and the importance of columns, as pointed out in the first part of the paper, must be kept in mind. Excessively high unit stress in a beam or slab may exhibit no visible signs of distress, but in a column the same high unit stress may wreck an entire building. Factor of safety and provision for safety are then things apart where columns are concerned.

Analysis, laboratory experiments, and experience with structures are the things that tell us what is the proper type of column to use and what proportions columns should have.

Unit stresses will be based on 2000-lb. concrete.

A column reinforced with either hoops or a continuous spiral will be called a hooped column. The hooped column is the one safe and dependable column in reinforced concrete, and, in general, this type of column should be used to the exclusion of others.

Hooped columns should preferably be round or octagonal in shape; if made square, the octagon in that square should be used for finding the area to determine the unit stress.

A standard type of column should be employed which has a balanced proportion of upright rods and coil, suited to the diameter of the column, and the pitch of the coil should be included in the standard.

The function of the upright rods and the coil should be considered as a cage to hold the concrete and give it a strength that is sure and a toughness that should characterize all columns. The unit stress should then vary with the length ratio of the column, but the only elements entering into that unit should be the full area of the column and the direct load. The function of the upright or longitudinal rods is to render the concrete a proper compression member. Tests have proven again and again that the percentage of longitudinal steel has little or nothing to do with the ultimate strength of the column.

A suggested standard of design for hooped columns is the following:

Make the coil of round steel having a diameter about one-fortieth of the external diameter of the column, and make the pitch of the coil one-eighth of the column diameter. Use 8 longitudinal rods of a diameter about one-fortieth of the column diameter.

For a unit stress on the full area of the circle or octagon between 10 and 25 diameters use $p=670-12\ L/D$ where p=unit stress in lbs. per sq. in.

L=Length in inches.

D=Diameter in inches.

Hooped columns should be limited in unsupported length to 25 diameters.

For columns of this type it is scarcely necessary to compute bending moments in the column, if the girders are calculated for a span center to

center of column, unless the case is unusual. If the girders are of long span, and particularly if they are shallow, and the load is all or nearly all on one side of the column, bending moments should be computed or steel columns should be used.

In building work designers seldom compute bending moments in columns. This is justifiable for all but unusual cases; for there would be no limit to the amount of work required, even in the design of a simple structure, if every conceivable case of bending were considered. There are scarcely any examples of balanced or concentric loading on the columns of a building. Tough columns, of the order of steel columns or hooped reinforced concrete columns make the computing of bending moments unnecessary except, as stated, for unusual cases. One of the unusual cases is that of a heavy girder load that connects to a column to one side of the center of the same. Another case where bending moments must be computed in columns is in exterior columns in girderless or flat slab construction, for the columns here must act as cantilevers to carry the floor load. I have found that, unfortunately, the practice is to disregard bending moments in these columns, and what is worse to use rodded columns for these. In flat slab construction the exterior columns must act as cantilevers to carry floor load. In this most critical of all locations in a building of this type the most unreliable of all types of columns is customarily employed.

Columns with square ties closely spaced could be used safely under restrictions both as to design and unit stress that have not heretofore been set up in any standard.

The restrictions as to design include:—a limiting ratio between length and diameter, total exclusion of the type in high buildings, employment only where loads are balanced or nearly so or where girders are short and deep and deflection plays a small part.

A suggested standard is the following:

Limiting ratio of length to diameter, 10.

Maximum spacing of ties, one-third of column diameter.

Diameter of ties not less than $\frac{1}{4}''$ with bent-over ends not less than $3''$.

In square columns, four rods of a total area about one per cent of the column area.

In oblong columns, six or more rods of the same size may be used, if each pair not ~~at~~ corners are tied across the column with ties spaced the same as the square ties.

Preferably all longitudinal rods should be of plain steel, for the concrete in shrinking will "slide down" a plain rod while it would more readily grip the deformed rod. Shrinkage stresses will thus be minimized where plain rods are used.

For a unit stress about 400 lb. per sq. in. is recommended in 2000 lb. con-

crete. This unit stress is to be computed on the concrete area only, no allowance being included for the steel.

In *Engineering* (London) of April 27, 1923, there is the description of an overbridge at Neepsend, Sheffield. This bridge illustrates a practice that does not seem to conform with British practice in regard to the spacing of ties in a rodded column. The arch bridge shown in this article is carried on sets of four columns at each end of the span. In fact two columns of each of these groups carry all and more than all of the load; for the arch is cantilevered over the inner pair of columns, and the dimensions indicate an uplift on the outer pair equal to the full load of the arch. This means double the load on the inner pair of columns. These columns are a foot square and have ties spaced 10″ on centers. The L. C. C. Regulations would restrict this spacing to six-tenths of the diameter of the column; also the R. I. B. A. Regulations seem to set the same limit.

There are other features of this bridge that illustrate the same confidence in the strength and toughness of reinforced concrete that has produced the large crop of failures. The design showed a hinged joint in the arch, which has a span of about ten feet less than the distance between the inner pairs of supporting posts. This, fortunately, was not made hinged in the construction. Whether or not the arch ring was reinforced top and bottom where the hinge was originally placed is not clearly shown in the description.

It has taken engineers a long time to grasp the lesson that reinforced concrete does not have the toughness which structural steel possesses, and articulation and deflection must be avoided in design or their effect provided against by appropriate means. This frequently precludes certain types of design.

Part III

The following contribution of mine to a discussion on columns is reprinted from the Journal of the Western Society of Engineers, June, 1913. It is given here for the lessons it carries on safe and sane methods of designing columns and analyzing column stresses and on correct interpretation and preparation of tests, also as an example of the efforts I have been making for more than two decades to stem the incessant tide of intricate mathematics that leads nowhere and clutters up the literature of engineering with material of no value to the man who does the world's designing, the man of all men whom this literature should serve.

A short time ago Prof. C. N. Ross of the University of Queensland, Brisbane, Australia, sent me, for discussion, a paper on columns which follows almost identically my paper referred to in the following discussion, which was published in R. R. Age-Gazette in 1909 and is repeated in my book "Steel Designing," published in 1913. The work of Prof.

Ross was independent of mine, but his conclusions are practically the same. It shows that engineers are beginning to see the necessity of simplification in these matters and of recognition of imperfection in columns as a prime factor in their design.

With the last paragraph of Professor Basquin's paper I am heartily in accord. The whole tenor of a paper of mine, published in the *Railroad Age Gazette,* July 2, 1909, was exactly along the lines of this concluding paragraph. I tried to urge upon the engineering profession that the essential thing to take into consideration, in investigating the strength of a column, is the imperfections "both with respect to its manufacture and with respect to the manner of loading." I recommended that it be assumed that in the manufacture of a column an eccentricity of 1/400 of the length is apt to result, and lack of centering of the axial loading may bring this eccentricity up to 1/300 of the length.

On this simple postulate a very complex formula was worked out, a formula entirely too cumbersome to use in practice. The locus of this formula was plotted, and it was found that up to about a value of 100 for $\frac{l}{r}$ the agreement with the commonly used straight line formula was practically identical; but little deviation was shown up to $\frac{l}{r} = 150$. The curve of this formula was peculiar, having so large a part of its length almost absolutely straight. In my deductions I did not assume that my column was frozen absolutely rigid with a bow of 1/300 of its length, as others have done, deducing results of no value, but I took into account the fact that the endwise load will augment this deflection by an infinite series. This series is vanishing for any fractional increment to the original deflection, but for a load that would double the original deflection, the sum is infinite. In this way it is proven that a load that will double any initial bow in a column, no matter how minute that bow may be, is the extreme ultimate load that the column can take. This is practically the Euler load. (Engineers have a vague idea that the Euler load is the buckling strength of a column. I have never seen a derivation of the Euler formula that demonstrated anything. Such derivations may exist, but the samples I have seen are not among them).

I submitted my deductions to the engineering profession as a contribution to the subject of the strength of columns and a substantiation of the "empirical" formula in common use, with the hope (a very faint one) that book writers, and others who come out periodically in print with exceedingly complex formulae for the strength of columns would

turn their attention to the imperfections of columns and of necessity simplify their mathematics, instead of covering these imperfections with intricate webs of so-called rational formulae, impossible of practical use.

The simplification of the mathematics that should logically follow a shifting from pure theory to consideration of imperfections is brought about by sheer necessity. There are endless ways in which a column may be imperfect or lack homogeneity, and the possibilities of the mathematician amusing himself along these lines will hold out until doomsday. The man who is designing columns for public consumption will demand a simple and workable formula. The simplest and commonest imperfection in rightly built columns is lack of straightness or lack of central application of the load. If a formula covers this, it is all that practical engineers can hope to have. As I stated in my paper, already alluded to, "a perfect column formula is neither a possibility nor a desideratum. If a perfect column formula were possible, only a perfect column would satisfy it."

A large part of the literature on the strength of columns could be rightly termed, "An investigation of the ultimate strength of carefully made nursery columns subject to endwise load, perfectly centered in a testing machine of the most perfect manufacture possible." This is especially true of literature on reinforced concrete columns. There is about as much relation between the condition of a concrete shaft in a testing machine and one in a monolithic building as there is between a life boat on a rolling sea and the arrangement by which students develop their rowing muscles in a gymnasium. There is not a great deal more of similarity between steel columns in a structure and in a testing machine. This is not written to discredit laboratory tests; they are eminently useful. It is written to discredit the standard method of arriving at deductions and conclusions, in other words, of interpreting these tests.

Interpreting the meaning of tensile tests is a simple matter. In a large measure it is true that if one square inch of steel in tension resists an ultimate load of x pounds, y square inches will resist xy pounds. Tension members even tend to minimize or overcome their imperfections under the force that tends to rupture them. To so large an extent is this true that the writer is familiar with some tests on bent bars that stood many times their calculated ultimate load, because the bars were tough enough to straighten out, partially, before failure. Compression members—particularly those of the lengths commonly used in structures —are quite the reverse. Imperfections are magnified by compression loads. If a column is bowed, application of the load will augment that

condition. Hence it is necessary to define sharply, comparison of tests and structures as between tension and compression members. Furthermore, there are certain details which, if not properly carried out, may make the results of tests of little, if any, practical value. Just as a beam tested without having its compression flange braced will give results that bear no relation to the strength of a beam that is properly braced, so a column not properly designed gives results that are of value only as they point out that such methods of designing columns should be eschewed.

Along this line, some tests on columns, described in Bulletin No. 44, University of Illinois Engineering Experiment Station, are on columns having web plates with a width between rivet lines 50 times the thickness of plate and lattice bars, with a distance between end rivets 70 times their thickness. Local crimping of these thin sheets gave stresses in lattice bars that represented transverse shears 2% or 3% of the axial load on the column.

Also this crimping caused failure at a low load. Now, specifications long ago required that plates be limited to 32 or 40 times their thickness as the width between rivet lines, and that lattice bars be not more than 40 times their thickness between end rivets. The engineering profession does not need these tests to prove that such flimsy plates and slender lattice bars are unsuitable for compression members. They worked that out long ago by experimenting with the elements of a column, and no amount of tests on full-sized columns can prove it any more than it is proven already by these experiments.

Tests of full-sized members should be on rightly designed members, as such design is now understood and practiced; then if any weakness develops in such design, the way to advance column design may be pointed out. The same bulletin describes measurements on lattice bars in an actual structure under stress. No measurable stress could be detected.

The engineering profession is not nearly so ignorant of the strength of full-sized compression members as some of its members would have us believe. It is true that the exact ultimate strength of columns (not slender) cannot be accurately told, but it is just as true that a perfectly safe column can be designed and built for any given load without wasting material, and this is the kernel of the matter so far as designers are concerned.

If more attention were paid to rational applications of theory already known in the design and proportion of columns, vastly more progress would be made toward safe and sane design than the never ending exhibits made with flimsy channels held at intervals by little batten plates,

and when one fails, an eminent authority says that "the use of tie plated columns, when the section is assumed to be integral, may lead to constructions which do not afford adequate security under loading of unusual character." The "unusual character" of the loading was that it was subject to the attraction of gravitation in a direction directly through the axis of the column. (See *Engineering News,* July 27, 1911.) An authority not so high could have asserted without the least fear of contradiction that these columns were abominable and dangerous and should not have been required to support any considerable load.

If writers busied themselves with showing when a column is of a certain ratio of slenderness and with emphasizing the fact that almost no columns in structures are really fixed-ended, engineering would be saved many a shamed face. It is here that the laboratory and the structure are differentiated. It is very difficult to get a true pin end in a testing machine. It is very difficult to avoid practically pin ended members in a structure. On the other hand, it is in the testing machine almost exclusively that fixed-ended members exist. Some members of a structure are continuous at their ends—held against free deviation of the tangent to their axis by other equally weak (or weaker) members—but about the only example of a fixed-ended compression member is a post abutting squarely on a masonry support. Many compression members are connected to gusset plates at their ends, and the gusset plates are supposed by some designers to hold the ends of the members perfectly rigid. A gusset plate in a roof truss (for an axis in the plane of the truss), is a better example of a "round end" or "pin end" in a member than a pin, since it allows almost unhindered deviation of the tangent to the axis of the member by bending the plate. Yet such members are considered by designers as being fixed-ended.

An intensely practical feature of column design that exhibited itself (by its absence) in the Quebec bridge wreck, and was totally ignored by the investigating commission, is the absolute need of strong lattice systems, in planes normal to the axis of the bridge, in the vertical posts beside the roadway, where swaying shears exist. When the great traveler fell over to the right and pulled this bridge down (as the investigating commission *did not report*), the whole top system was readily pushed to the right, since the lattice in all the verticals was so flimsy. The verticals crumpled in *S* shape. A little lateral stiffness in the posts might have broken the traveler in place of the bridge.

Investigators have erred in failing to appreciate the fact that there are two distinct phases to the strength of a column. One has to do solely with the modulus of elasticity and is totally independent of the elastic limit or ultimate strength of the material. It has application to slender

columns. The column fails as a spring or bow at the first measurable deflection, and all grades of steel from the softest to the hardest will fail at the same load (the Euler load), since their moduli of elasticity are practically the same. Even imperfect columns, with a very considerable initial bow will have the same strength as perfect ones, and this ultimate strength can be very closely calculated. These are some of the anomalies on the subject.

The Euler formula has little practical use in structural design, because slender columns are tabooed by specifications. In one way, however, it has a bearing on specifications that have to do with nickel steel. Formulae for the strength of soft steel and nickel steel should converge to practically the same value in high ratios of slenderness. This is not the case in some recent specifications.

In spite of the inapplicability of the Euler formula to other than slender columns, this formula is commonly used by European engineers for columns that are not slender. There is no excuse for this.

The other phase of the strength of columns has to do with the crushing strength of the metal or its elastic strength. It has application to short columns. The Gordon-Rankine formula comes near meeting the requirements of this class of columns, but it has two faults or qualities that are apt to give rise to faults or errors. One is that the values do not fall away sufficiently rapid at the start. Very short columns do not have strengths comparable with short blocks or cubes. This may be due to wrinkling of the metal. It is a fact just the same that needs expression in a practical column formula. The other fault is that unless the constant is such as to keep the load for slender columns below the Euler load, the formula is useless and misleading for long columns. The straight-line formula avoids both these faults and is therefore better suited to practical use.

CHAPTER IV

"REINFORCEMENT FOR SHEAR IN REINFORCED CONCRETE BEAMS."

By Edward Godfrey, M.I.Struct.E., M.Am.Soc.C.E.

Paper read before The Institution of Structural Engineers, 296, Vauxhall Bridge Road, S. W. 1., on Thursday, 22nd February, 1923, at 7.30 p. m.

Reprint from Jour. Inst. Struc. Engrs., March, 1923.

In every beam there are three principal elements that must be taken care of in the design before the beam can be considered properly designed or safe. One of these elements is provision for tension in the bottom part of the beam in the portion where the bending moment is the maximum (assuming a positive bending moment). In a reinforced concrete beam this is effected by the steel rods which must lie near the bottom of the beam where moments are high and must extend to the ends for grip or anchorage.

The second element is provision for compression in the upper part of the beam. This is taken care of by supplying width and depth of beam of sufficient magnitude to keep the unit stress on the extreme upper edge within safe limits.

The foregoing is mechanics of the most elementary character. There was never much discussion or dissent on the two propositions already stated except as they affect the extent of provision for these stresses. Such matters as the intensity of stresses permitted in safe design and location of the neutral axis and variation of stress intensity have been the subject of controversy, but these matters deal in reality with the question—How much? No engineer or authority has ever questioned the need of lower tension reinforcement continuous in character and extending to every part of the beam in bending. Nor has any engineer or authority questioned the need of upper compression area to match in concrete the tension steel.

The third requisite in any beam is provision to take the shear in every part of the beam, particularly that to take the end shear to the support. It is in provision for the shear, or lack of provision for it, where the great errors in design are made and where the cause of many failures is located.

Analysis of shear and of methods of shear reinforcement in concrete beams is one of the most illogical things in all the literature on this subject. Standard methods of taking care of shear in reinforced concrete beams are uniformly illogical and inefficient.

Commercialism has had much to do with this illogical feature of reinforced concrete design, in America at least. Patents have been taken out on systems of reinforcing beams, and with the impetus of development along commercial lines unscientific tests have been made and unscientific interpretation of the same published.

Much of the erroneous reasoning and false methods of design has been the direct result of the use of analogy in working out the problems of reinforced concrete. The truss idea of steel construction has been adopted by the reinforced concrete designer and authority, but they have failed to observe that their truss web members, the stirrups and short shear members in reinforced concrete beams, lack vital essentials that must be in every truss.

Another false line of reasoning employed by advocates of stirrups is analogy with built-up wooden beams—stirrups being assumed to act as pins or bolts.

Analogies are sometimes dangerous things to work with. They are apt to lead to wrong conclusions, especially when applied to materials of other properties and to cases where some vital conditions are lacking.

It is proposed to show here that the analogy which asserts that a reinforced concrete beam is a modified Howe or Pratt truss is totally false and that the one that asserts that stirrups act in a concrete beam as bolts or pins is also totally false. As the standards of designing for shear in reinforced concrete are based on these two ideas, these standards are on a false basis and hence should be discarded.

Still another line of reasoning applied to the designing of shear reinforcement is a theoretical one that attempts to discover the intensity of shear in the concrete for the purpose of spacing the stirrups. This is also a blind type of reasoning that falls short of any conclusion as to what becomes of the stress in the stirrup or how it can meet any adequate resisting medium. In fact, all of the three lines of reasoning referred to have this common fault, namely, they apparently discover a stress in a stirrup or short shear member, but they fail utterly to follow out that stress to an adequate reaction. As I shall show, it is exactly as though one should discover certain stresses in the web members of a truss and section enough to carry those direct stresses, and on this bare premise declare the truss to be safe, ignoring altogether the fact that the end connections of these members are totally inadequate to take the stresses.

I am sometimes accused of setting up straw men and then knocking them down, so if I fail to cite references in speaking of the standard analyses for shear, this accusation may be used against me. I shall, therefore, cite some references to substantiate my assertions as to the nature of standard analyses for shear and standard methods of reinforcing for shear.

Taylor and Thompson, in "Concrete, Plain and Reinforced," recognise only three methods of reinforcing for shear, namely, vertical stirrups, short

diagonal rods, and bent-up portions of main rods, bent up at intervals but not anchored over supports nor even reaching the supports. The analysis of shear in concrete given in this book is a purely theoretical analysis, which takes the infinitesimal cube and discusses the need of the material concrete of reinforcement against diagonal tension or shear. It is an analysis which ignores the beam, but merely discovers the need of reinforcement in the concrete itself. The authors run away from actual analysis of the beam by stating that shear is a measure of diagonal tension, and "this measure has been universally accepted."

The logical deduction from this line of reasoning is that concrete in high shear should have a very large number of very small steel wires closely spaced to effect reinforcement.

In "Walls, Bins, and Grain Elevators," by Milo S. Ketchum, p. 516, a solution is given to the stirrup problem. This is illustrated by Fig. 1 (to which has been added the line XX).

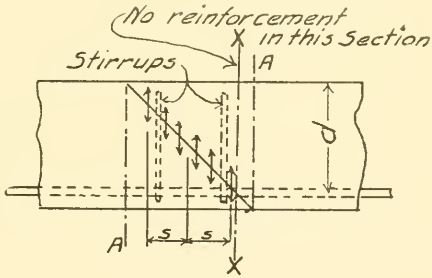


Fig. 1.

The concrete of this beam in the design has already been given a value in shear. The surplus of the shear of the beam is assumed to be carried by the two stirrups that are cut, each taking one half, in tensile stress on these stirrups.

Faber and Bowie in their book, "Reinforced Concrete Design," published in England, give the usual treatment of shear reinforcement common in American books. They show Kahn bars as a standard reinforcement as well as other patented types. On page 316, where the Report of the Royal Institute of British Architects is quoted, only stirrups and short shear members are contemplated as shear reinforcement, and these authors say that this Report is "probably one of the best of those used in the English tongue." On page 86 they state that the stress in stirrups is almost pure tension, and not shear as occasionally stated. Further, these authors give the same rule for finding the stress in stirrups or for spacing them as that indicated in the foregoing paragraph, given by Ketchum. It is to be noted that on page 56 they figure the whole length of a stirrup for anchorage if one end is looped.

In "Principles of Reinforced Concrete Construction," by Turneaure and

Maurer, ed. 1907, paragraph 125, sanction is given of the use of stirrups by a vague rule which would place enough stirrups in the depth of a beam to take the shear of a beam at a "working stress" of 15,000 lbs. per square inch.

Buell and Hill in their book do not say anything about stirrups, but show examples of beams with stirrups in them.

In "Reinforced Concrete Design Simplified," by John C. Gammon, p. 101, vertical stirrups are figured for shear at 12,800 lbs. per square inch. Diagonal stirrups are figured for tension at 16,000 lbs. per square inch.

Marsh and Dunn in their book, 1909 edition, show the Khan bar as a standard shear reinforcement for beams. On pages 148 and 149 they speak of effecting shear reinforcement by bending up some of the bars and by using vertical rods. Also they illustrate the use of stirrups. The engineering instinct of these authors seems to commend the use of bent-up and anchored rods, for they show a type of beam having such rods on page 153, but this is used only in conjunction with stirrups, and no rule for designing such rods is given.

Webb and Gibson, 1920, mention only stirrups and bars bent up at such angles as 30 to 45 degrees as reinforcement for shear. They apologise for the lack of rationality of the method of bending or spacing the bars.

E. Lee Heidenreich, 1915, on page 73, refers specifically to two methods of reinforcing for shear. "One of these consists in bending all, or part, of the longitudinal bars up toward the supports at various points. The other method involves the use of stirrups, either vertical or inclined, which should be attached to the main reinforcement."

On page 74 he states that it is necessary that the stirrups have sufficient length of embedment above the neutral axis to develop the requisite stress. Mr. Heidenreich shows examples of web reinforcement where main bars are bent up at intervals but not carried to the support; he also shows some examples where bent-up bars are carried to the supports and anchored over the supports. He fails to mention the latter in the text in any explanatory way.

Sabin, in "Cement and Concrete," commends a patented system for the "thorough manner in which all tension stresses are provided for." He compares the shear members to the tension diagonals of a truss, and speaks of the way in which the stirrups take shear in each "imaginary" panel of the beam. He says nothing about what the "floating" ends of these members do with that shear.

In "Concrete and Reinforced Concrete Construction," by Reid, pages 309, 310 and 311 indicate clearly that this author would proportion vertical stirrups as pegs to take the horizontal shear. If the increment of stress in the reinforcing rods is too much for the concrete, it is plainly stated that vertical stirrups may be added to supplement it. The stress in the stirrups is treated as actual shear. This book gives a large number of illustrations to guide the

designer where stirrups are used. Some of the stirrups are vertical, some are inclined at an angle of 45 degrees with the horizontal, some are at a greater angle, some are loose, and some are attached to the horizontal rods.

In "Trans. Am. Soc. C. E.," Vol. LXX., on the pages following their names, the following named engineers compare a stirrup or short shear member system to either the Pratt, the Howe, or the Warren truss, or all three: S. Bent Russel, p. 73; J. R. Worcester, p. 75; Edwin Thacher, p. 85; Paul Chapman, p. 91; E. P. Goodrich, p. 96; John C. Ostrop, p. 108; John G. Sewell, p. 125.

Mr. Duff Abrams, in "Concrete" (Detroit), August, 1920, says: "It should be understood that these illustrations were generally for the benefit of beginners, and should not be considered as evidence of ignorance on the part of designing engineers." I never did believe in stuffing children with Santa Claus stuff and then having to take it all back again. Besides, engineering babes are not seeking milk in the Transactions of the American Society of Civil Engineers.

In a series of lectures by Dunn, published in England, the author shows parts of main bars bent up at 45 degrees at regular intervals and looped over an upper horizontal bar. "The beam may then be regarded as a lattice girder; the sloping steel rods should be sufficient to take the total shear at each point, or the total shear less the amount taken by the concrete or less the amount taken by the concrete and stirrups, if any."

Hool and Johnson, in "Concrete Engineer's Handbook," 1918, give much space in Section 7 to description of methods of spacing vertical stirrups and bent-up short ends of main rods. On page 296 is a figure showing where these rod ends need not reach the ends of the beams nor have any anchorage over the supports. Farther on in this book these authors use bent up rods, in connection with stirrups, but always the bends are at an angle of about 45 degrees, and many of them are away out from the support. In some cases the rods reach to the supports and lap over the same, which is a splendid form of anchorage.

Lewis and Chandler, in their book published in 1919, p. 171, show the truss idea. They also show figures with vertical and inclined rods as stirrups, and they indicate what was formerly much used in advertising propaganda by owners of patented systems, namely, a beam masquerading as an arch with abutments at every intersection of a stirrup with a horizontal rod. Perhaps this is only intended to lead on the beginner, and not to be taken seriously.

Mr. C. A. P. Turner, in "Concrete" (Detroit), October, 1920, p. 119, says of stirrups: "They simply resist horizontal shear, which is the theory and function of the vertical stirrup." Over against this place the remarks of Mr. Sanford E. Thompson, in the same periodical, August, 1920, p. 45, where he intimates that stirrups are not now figured for shear, though years ago they were so figured. And against this place the following in Mr. Thomp-

son's own book, edition of 1916, p. 516: "In case the concrete in a beam or slab has cracked vertically next to the support because of accident or poor design, the bearing value of the horizontal rods may have to be estimated." If this does not mean figuring steel rods for shear, it does not mean anything. And what about a design where the horizontal rods do not reach the bearing, as they are not required to do in any but the very latest regulations.

Mr. C. A. P. Turner says in "Concrete" (Detroit), October, 1922, p. 109: "Direct tension in the steel, compression in the concrete, and shear resistance to sliding are the three elements of stability." This is his statement of what is required in a reinforced concrete beam.

Prof. F. E. Turneaure, in his book, 1919 edition, p. 129, says, and emphasises it in italics: "Horizontal rods only are not adequate reinforcement against diagonal tension."

The standard systems have nothing but horizontal rods between stirrups. What, then, takes the diagonal tension between the stirrups that are ¾d apart, as allowed by Prof. Turneaure in this book and as allowed by the Joint Committee and the American Concrete Institute?

Prof. Turneaure on page 106 states that the ultimate shearing strength of 1:2:4 concrete is about 125 to 150 lbs. per sq. in., a safe value being 40 lb.

The Joint Committee Report, a report issued by a committee of about thirty engineers appointed from a number of engineering societies in the United States, is held as the standard of practice in America. This report never had any provision whatever for any other kind of shear reinforcement except stirrups and short bent-up ends of rods until the latest tentative report issued in 1921, in which, at my insistence (I was not a member at that time, but I appeared before the committee for a day's session and urged this), they added an alternative reinforcement with bent up and anchored rods. In 1916 I was a member of this committee, and refused to sign the Report until allowed to incorporate in the same a dissenting note dissenting from every word concerning stirrups and short shear members and rodded columns.

The American Concrete Institute has paralleled the Joint Committee in this matter, giving no recognition whatever to any type of shear reinforcement but short, so-called, shear members, with the exception of my recorded minority reports and dissenting notes; until within the last year or two when they have amended their standards, on my urgent appeal, and allowed as an alternative real shear reinforcement.

Prof. W. K. Hatt, in "Proceedings, American Concrete Institute, Vol. xiii., p. 285, 1917," says, "The principles of action of web reinforcing are on a very insecure experimental basis and have little rationality." In justice to Prof. Hatt I wish to say that when in 1920 I expressed my commiseration for any professor who had to teach from or apologise for standard works on this subject, he repudiated the statement of 1917, but he did not make any

effort to show in what way he would revise it. The new light he professed to have was not revealed.

In "Engineering News Record," Feb. 27th, 1919, Mr. W. A. Slater describes a series of tests that have been the basis of radical changes in recommended practice in America. The beams tested were of I-beam shape with thin webs and heavy heads nearly square in shape (the heads), and they were heavily reinforced with close-spaced vertical and diagonal rods well anchored into these large concrete heads. The longitudinal rods were anchored with large loops into the heavy end posts. Among the tests there was one without any concrete web. This one stood a load about half as great as those with a three-inch concrete web surrounding the steel reinforcement. In spite of this good showing of the test without a concrete web this experimenter, in the tabular list of his tests, shows apparent ultimate shear values on the ones with concrete webs which are equal to the shear of the beam divided by the area of a nominal concrete web, namely, a rectangle whose dimensions were

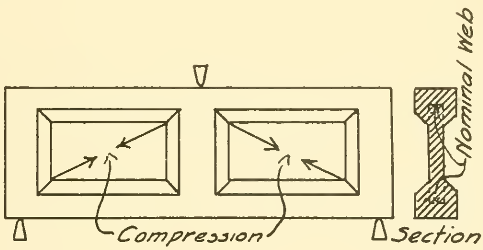


Fig. 2

the thickness of the web by 30 inches. The beams were 36 inches deep and had a total cross sectional area about double that of this nominal web, and every square inch of this area was of necessity in shear while the beams were whole. Of course, by this method of figuring large ultimate unit shears were found, and by making the web thinner and thinner they would have "approached infinity," as the mathematicians say. The one without a concrete web, on this system, carried a unit shear of infinity. The concrete webs were in fact practically relieved of all shear before failure, for the webs were shattered long before the beams failed and incapable of taking shear. Diagonal compression, as per Fig. 2, completely accounts for the alleged high shear in the webs.

In spite of all this, which I publicly pointed out, unit concrete shears as recommended in America, were doubled because of the showing of these "impossible" tests.

It is interesting to note that this set of beams began to show failure (cracks) in the concrete webs at loads of between 200 and 300 lbs. per sq. in. on the nominal web area, something over 100 lbs. per sq. in. on the actual

concrete area subjected to shear. Twelve per cent. of the ultimate compressive value, the unit stress now proposed to be used in design, on the strength of these tests, is about double this. And the cracked concrete web is supposed to take a large share of this, alleged steel reinforcement being supplied for only a portion of the beam shear.

Does anyone wonder why in six months thirty-one columns of one of our engineering periodicals were filled with description of reinforced concrete wrecks some years ago?

In "Proceedings American Concrete Institute," vol. xv., 1919, Mr. W. A. Slater gives an analysis of the stresses in web reinforcement composed of two systems of close-spaced stirrups, connected to the top and bottom horizontal reinforcement, each system acting independently of the other and of the concrete in which it is embedded. Mr. Slater seems to find a rough agreement between this double-intersection truss analysis and his tests referred to in a foregoing paragraph. It is almost superfluous to point out that only in a shattered beam could the independent action of the steel system and concrete exist.

In other examples of tests cited to substantiate the truss idea in analysis, or to show the benefit of stirrups, the stirrups are profusely distributed in the beam. Mr. S. E. Thompson, in his book already referred to, states in two places: "Numerous tests have demonstrated that a beam properly reinforced with stirrups or bent bars sustains three or four times as much load as the beam without web reinforcement." In his book he recites tests as follows:— One set of short beams, with 11 stirrups, shows an increase of 58% instead of 300% to 400%; one set of short beams, with 38 stirrups, shows 80% increase; another shows 43%; another 36%. Among tests made by Withey (Bulletin 175, University of Wisconsin), he shows on page 42 an average unit shear of 158 lbs. for tests 1, 8, 9, 10, 11, without web reinforcement, and an average of 160 lbs. for 4, 5, 6, 7, with alleged web reinforcement.

The tests referred to in the last paragraph with 38 stirrups means 76 upright rods in 13 feet. Compare this "horse comb" and the tests by Slater with their close mesh of anchored stirrups with standard methods of spacing stirrups in beams. The Ransome rule is to make the spaces ¼d, ½d, ¾d, etc. (d being the beam depth). Several of the authors cited in this paper give that rule. The Joint Committee and the American Concrete Institute stated until very recently that no stirrups are needed in the space ½d from the ends, and they both stated that beams do not break in that ½d zone. I publicly proved that beams do break in that zone, both laboratory beams and, in large number, beams in wrecked buildings. (See "Concrete" of Detroit, September, 1920, pp. 87, 88, and same, October, 1920, p. 118.)

One of the classic American wrecks had beams with no steel reinforcement in the half-d zone adjacent to walls and columns. I criticised this feature publicly. The builder, a prominent owner of a patented system, told

me in a sharp letter that he did not want to discuss the question with me, but he proposed a fool's argument, a wager. Here were the conditions: He said he was going to rebuild exactly as it was before the collapse. (Our building inspection bureaux would not allow exactly the same blunder to be repeated in exactly the same place.) He said he was going to test the building and proposed that I pay for it if it stood the test, and he would pay for it if it failed. The reader can make his own deduction.

Very recently there has been added in the Joint Committee Report and Standard Practice, American Concrete Institute, a provision that when shear is as high as 6% to 12% of the ultimate compressive strength of the concrete on the section of a beam (an unheard of high value), main steel reinforcement must be anchored into the support. This has one encouraging phase, namely, it recognizes the benefit of end anchorage of main reinforcing steel as a carrier of the end shear of the beam. Before this provision was introduced into these American standards there was nothing in either of them that required that any steel whatever cross the half-d zone, at the end support of any reinforced concrete beam. I fought for fifteen years for recognition of the necessity of anchoring steel.

Referring to the high shear value proposed to be allowed in the Joint Committee Report, it is interesting to observe that in a footnote in the Report it is intimated that this value is moderate, and it is stated that it could be larger than 12 per cent. if inclined web reinforcement be used. The footnote says that beams reinforced with vertical stirrups fail at 50 per cent., *"due to diagonal compression in the webs."* The experimenter who made the tests illustrated in Fig. 2, and who was on the Committee which wrote this Report, made no mention of "diagonal compression in the webs" in his description of the tests, and he did intimate that the *shear* carried by the webs was about *100 per cent.* of the compressive value of the concrete. I publicly pointed out that, as indicated in Fig. 2, his beam shear was carried by diagonal compression, and that one test beam without a concrete web was able to carry half as much, or more, as some with a concrete web. But why risk lives and property by basing actual design on the showing in tests of anything so unlike any part of a structure as these concrete I-beams? This is said apart from the observations already made that the concrete actually in shear was double the nominal web, and the failure started at the ordinary ultimate shear unit not much over 100 lbs. per sq. in. If it had not been for the unusual character and heavy anchorage of the stirrups of these beams, they would have failed at a fraction of the load that they did sustain. The one thing that their strength cannot be attributed to is the webs as measured by their shearing area.

In the midst of all of this contradictory and illogical mass of information concerning shear reinforcement we find occasional mention, as though it were an exhibit in the museum of engineering, of the Hennebique patent. This

patent is dated October 4, 1898. The first claim describes correct reinforcement for shear, and makes no mention of stirrups. Until I began to look up data for this paper I did not know this, though I have been for fifteen years publicly contending for this type of reinforcement. Any Hennebique floors that I have seen illustrated have always had, as has his patent drawing, stirrups as well as bent up and anchored rods. A large building which I tested, of Hennebique construction, had rods bent up to the top of the beam at the ends and anchored, as well as other rods bent up to the middle of beam and anchored: besides this there were stirrups. I can see no justification for wasting steel by bending up rods to the middle of the beam.

Hennebique's patent has expired, as also that of De Man, granted in the same year but prior to Hennebique's, and showing similar reinforcement. These patents were in a measure anticipated by an English patent granted to Hyatt in 1877. Patent examiners seem to grant patents regardless of novelty or priority, and leave it to litigants to fight it out.

In spite of the paucity, in standard works and recommended practice, of any direct information on the subject of bent up and anchored rods, we frequently see designs where engineers have used this type of reinforcement for shear. In this respect designing in reinforced concrete transcends the standard literature of the subject.

Turning now to tests and their showing. It is freely admitted that where there is a large number of closely spaced stirrups, a beam will be stronger by reason of these stirrups, especially if the main horizontal rods extend beyond the edge of support. Reference has been made to some of these tests already, and it has been pointed out that these tests are so far from being similar to actual construction, or to what is called for by the standards of design, that their value is little or nothing. In fact some of these tests are worth less than nothing because of the misleading character of the interpretation placed upon them by the experimenters.

Prof. A. N. Talbot, in Bulletin 29, University of Illinois Experiment Station, describes a lot of tests made to determine the value of stirrups. The following is quoted from that Bulletin: "Until the concrete web failed in diagonal tension and diagonal cracks have formed there must be little vertical deformation at the plane of the stirrups, so that not much stress can have developed in the stirrups.......It is evident, then, that until the concrete web fails in diagonal tension little stress is taken by the stirrups....... It seems evident from the tests that the stirrups did not take much stress until after the formation of diagonal cracks.......It seems evident that there is very little elongation in stirrups until the first diagonal crack forms, and hence up to this point the concrete takes all the diagonal tension.......After the diagonal cracks become visible the stirrups take tensile and bond stresses, and the diagonal cracks extend and enlarge."

In this Bulletin, Prof. Talbot tells how to proportion stirrups and indicates

how much of the shear may be counted on as being carried by the concrete web. It is interesting to note that where main bars were bent up with a flat bend and anchored over the supports, the results were very good in the shear carried, and the beams *failed by tension in the steel.* Beam 229.8 is a direct comparison between stirrups and main rods bent up with a flat bend, as one end had stirrups and the other had the bent up rods, which were not even anchored. The first crack was in the stirruped end, and failure occurred in that end. The beam was symmetrically loaded.

Bulletin 64 of the University of Illinois Experiment Station, by A. N. Talbot and W. A. Slater, gives some still more striking proof of the benefit of rods bent up with a flat bend and anchored over the support and the error of using stirrups and bent up rods that are bent up with a sharp bend and not anchored over the supports. These tests were made on buildings. Where the alleged shear reinforcement was stirrups and bent up rods not anchored, the stirrups and the bent up portions of the rods were found to have a small compression when the floors were loaded, and the beams were cracked. Where the rods were bent up with a flat bend and anchored over the supports, large tensile stress was found in these rods, and no cracks were found in the beams. These rods were bent up with an angle less than 20 degrees with the horizontal, a detail that, in general, I would insist upon. The Joint Committee, 1921, Report, requires the angle to be greater than 20 degrees. This part was written by Mr. Slater, who made the tests just referred to.

I have made a number of tests on large buildings where the shear reinforcement was by means of main rods bent up at angles less than 20 degrees and anchored over the supports. The results were excellent, and no cracks were found in the beams. I have also tested other floors of large buildings where the shear reinforcement was vertical stirrups. The results were exceedingly bad, and cracks in the beams were very numerous, and some of them very wide open.

To sum up the situation as regards shear or diagonal tension reinforcement and analysis of the same:

(a) There are engineers and recognised authorities who hold that steel rods embedded in concrete can take shear at units such as 10,000 to 12,000 lbs. per sq. in.

(b) There seem to be no standard specifications that allow such shear, or any stated shear units, on steel rods embedded in concrete, though such were common fifteen years ago, when I began publicly to demonstrate that the thing is absurd.

("Concrete Engineering," January 1, 1907, and my book "Concrete," 1908, pp. 7, 208, 366.)

(c) There are engineers and authorities who say that no such allowances can be made on steel in concrete. There are those who aver that this has been entirely abandoned.

(d) Engineers and standard authorities are almost a unit in their adoption of the following three methods of reinforcing a beam for shear or diagonal tension:—

1. Vertical stirrups.
2. Stirrups inclined at about 45 degrees with the horizontal.
3. Bent-up ends of main rods, bent up at angles of about 45 degrees, the rods ending in the upper part of the beam.

All of these methods are by short shear members. The shear members are inclined at a large angle with the horizontal. Whether they are connected by means of loops or by being part of the horizontal bottom rods is a detail that does not affect the principle very much. The thing that stands out in all standard practice in reinforced concrete is that shear is to be provided against by short shear members, and only rarely or recently are those members required to have any semblance of anchorage into the supports. Some authorities have recently got under cover in this matter.

(e) Many designers and some authors, seeming to sense the futility of disjointed short rods as a reinforcement for shear, throw in for safety main rods bent up with a small angle and anchored over the supports. Very recently my fifteen years' fight for recognition of this type of shear reinforcement, without stirrups, has resulted in its inclusion with other types in the regulations of the American Concrete Institute and in the Joint Committee Report.

(f) Tests prove that in a whole beam stirrups and bent up rods of the standard type receive no stress whatever of any consequence. They may even receive a small compressive stress, which is the very thing that they are not designed to do. Tests also show that when the beam is broken up by shear or diagonal tension cracks in the concrete, the short shear members are of course subject to tension. Why tests should ever need to be resorted to to discover this is a mystery.

(g) Tests have proven that when a beam is reinforced for shear or diagonal tension by bending up main tension rods to the top of the beam at supports and anchoring these rods beyond the edge of support, the bent up portion of the rod being at an angle of 20 degrees or less with the horizontal, excellent results are obtained. The rods are found to take large stress while the beams are whole, thus proving that they are doing the work expected of them, and the beams are frequently not even cracked under high loads. And this is the type of reinforcement not even permitted by the latest American standard (the Joint Committee Report), though that report does allow, in its latest addition, a steeper angle to reinforcing rods than 20 degrees. Standard works in America and England make no mention of this method of reinforcing for shear, excepting that they hint that it may be used as an extra precaution. Before going into detail in the matter of further

demonstrating the error of the stirrup I shall illustrate and explain true reinforcement for the shear of a reinforced concrete beam.

Figs. 3 and 4 show rational and economical reinforcement for the shear of a beam. By the shear of a beam is meant the sum of the vertical forces which on one side of a vertical section are upward and on the other side of that section are downward. It matters little whether in the concrete itself the stress be interpreted as shear or diagonal tension, the big thing is to provide reinforcement that will safely and economically resist the shear of the beam or at least that part of it which is beyond the capacity of the concrete itself to carry.

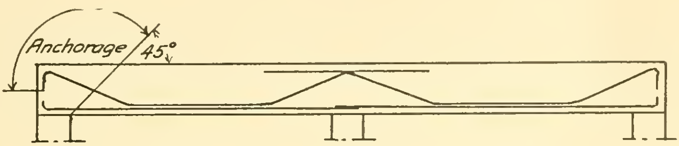


Fig. 3.

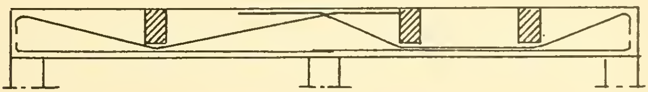


Fig. 4.

That the type of reinforcement shown in Figs. 3 and 4 is economical is beyond dispute. That it saves a lot of time and trouble in the field as against a mass of small stirrups or shear members is equally evident. That it is a type of reinforcement that can be completely analysed and fulfils every condition of rationality has never been questioned. That it has proven itself to be the surest and best reinforcement against beam failures due to high shear is a matter of record proven by tests made by men who never publicly recognised it as even an alternate method of reinforcement until the most recent time.

The elements of this reinforcement are very simple. In the first place a beam that has the main steel anchored into the supports both at top and bottom of the beam is most favourably conditioned as regards the ability of the concrete itself to take shear: for even if the concrete should crack, the roughness of the surfaces, and the reinforcing rods which hold these surfaces firmly together, would enable the beam to carry a large shear. It is therefore admissible to allow a good shear unit on the concrete. A unit shear of 60 lbs. per sq. in., or even more, can safely be allowed on the full section of the rectangle of the beam. The shear of the beam in excess of this amount should be taken as a direct stress in the inclined portion of the main steel rods, which are bent up and anchored beyond the edge of the support. To find the stress in the bent up rod or rods multiply the excess of shear over that carried by the concrete by the length of the inclined portion of the rod and divide

by the vertical projection of the same (in Figs. 3 and 4), or, in other words, multiply by the secant of the angle the inclined rod makes with the vertical.

In a beam carrying uniform load, the rods should be bent up at about quarter points in the span. They should, of course, be bent up at points not nearer to the support than the section of the beam where the shear equals 60 times the area of the cross section. This will generally give or allow a small angle with the horizontal. There has never been given any published reason why this angle should exceed 20 degrees, as required by the Joint Committee Report, except the publicly stated personal view of the man who put the defenceless limitation in the Report. It is a fact that the report submitted to the American Concrete Institute in 1920 prohibited any reinforcement for shear that was bent up at a smaller angle than 30 degrees with the horizontal. This was changed before the report was adopted, because of my urgent protest.

In beams carrying concentrated loads, as in Fig. 4, the rods should be bent under the beams, or approximately so.

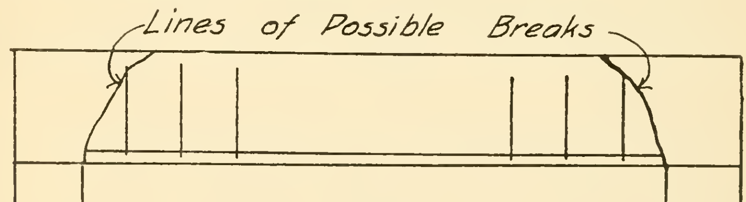


Fig. 5.

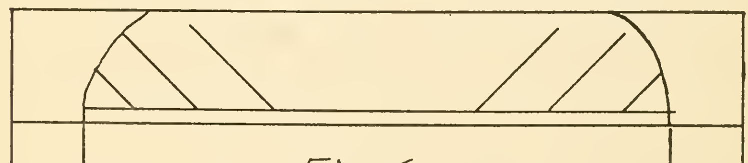


Fig. 6.

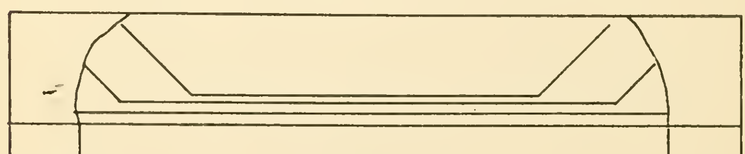


Fig. 7.

The bent up rods should be anchored into end supports as indicated in Fig. 3, or by some other equally efficient method. If the length of rod beyond the 45 degree plane is about 40 or 50 diameters, the anchorage should be sufficient.

Turning now to the stirrup or short shear member, and the arguments against the same, the first argument is a set of figures showing what for many years has been considered standard and safe design for shear reinforcement of beams and showing the lines where these beams can fail *by shear* without disturbing the alleged shear reinforcement.

Figs. 5, 6 and 7 are these figures.

These figures have been published before as an argument against short shear rods. Let us examine the response to this argument.

Mr. John S. Sewell, Trans. Am. Soc. C. E., Vol. LXX, 1910, p. 124. Referring to a sketch similar to Fig. 6 where the angle of the bent up rods happened to be 60 degrees, he says "The angle at which the bars are bent up is rarely as great as 45 degrees, much less 60 degrees." It will take someone wiser than myself to point out how this beam could be declared properly reinforced if the angle of the short rods is 10 or 15 degrees more or less, when the whole beam can sever from its support no matter what the angle is.

Mr. S. E. Thompson in "Concrete" of Detroit, August, 1920, p. 45, "A careful consultation of modern practice and modern textbooks would have shown that the inclination of bent bars advocated and used in practice is 30 to 45 degrees with the horizontal." Sixty degrees is here again objected to by a man who in his own book advocates the use of vertical stirrups, where the angle is 90 degrees.

Mr. W. G. Thomson, in "Concrete" of Detroit, August, 1920, p. 44, "The cracks are always drawn so as to conveniently miss the stirrups, yet in no single instance, that the writer can recall, has there been presented any real data in support of his position." I wonder if the man who wrote the sentence just quoted ever heard the expression, "the weakest link in the chain." As to the published data in support of my position, I could quote several pages of it, where after wrecks have occurred I have pointed out publicly the fact that either the columns were rodded columns and broke up in chunks, or the beams were stirruped beams and fell away from the supports exactly as Figs. 5 to 7 illustrate, or were a combination of both these errors. These big facts of engineering experience are by some men considered of no value whatever: only the carefully nursed laboratory tests on things that bear little or no resemblance to structures as built are considered as proper data or facts.

Imagine a contractor trying to pour concrete around seventy-six little upright rods in a beam 13 feet long, and to hold all of these rods in place during the operation. Yet this is one of the classical tests that have been referred to time and again as part of the facts and data which, they say, I ignore and which, they say, prove the value of vertical stirrups.

Formerly there was no demand for stirrups to be anchored either by embedment in the concrete or by attachment to the horizontal steel. Now there is some recognition of the benefit of tying the beam together by anchoring and connecting the reinforcing steel. The authorities already referred to in

the early part of this article call for anchorage in the following ways:

1. Some authorities say that stirrups should have an embedment in concrete that will develop their full strength above the neutral axis.

2. Some authorities say that it is sufficient if in the full length of the stirrup the strength is developed, one end being looped around the bottom steel for the reaction.

3. The Joint Committee in its latest Report calls for looping around horizontal steel at bottom and at top of the beam in cases where the unit shear is exceedingly high.

All of these authorities must, of necessity, use the truss analysis to account for the stress in the stirrup. No other analysis has ever been set up.

Let us examine these methods of anchoring stirrups in an effort to answer the question, "What becomes of the stress in a stirrup after it leaves the stirrup?"

Take a stirrup that is looped around a horizontal rod, say the stirrup is a ½ in. square rod. The alleged stress is 8,000 lbs., a vertical force. What becomes of it? It is a force that must reach the support of the beam. Is it taken by shear in the horizontal rod? This is impossible and absurd. And yet there is no other medium but the rod, and the concrete which is supposed to be relieved of shear by the stirrup, and absolutely no way in which it could be taken except by shear in that rod.

Next, consider the stirrup which requires its full length for embedment to effect anchorage. How can this be considered a vertical member. It has its full stress in a length of zero.

Next consider the case of the stirrup that has its full anchorage above the neutral axis. What about the stress or the shear of the beam above the neutral axis? How can a stirrup be considered as a vertical member in a truss if its effective length is only one half of the depth of the truss?

The case of the stirrup that is looped around horizontal rods at top and bottom of the beam is again that of a right angle pull against a rod which cannot be taken in any other way than by shear in the horizontal rod. Besides this absurdity the upper horizontal rods are put there to take tension. How can the vertical pull of the stirrup be converted into horizontal tension in the top rod? No advocate of stirrups has ever attempted to reply to this sober and searching analysis of the stirrup. The same analysis applies, with a little variation, to all short shear members—to all systems that employ sharp bends in steel rods in tension. The right angle bend (the vertical stirrup) or the sharply inclined bar (diagonal stirrup) cannot be defended.

One of the strongest proofs of the error of the stirrup is the character of the arguments made in defence of the stirrup and in a large measure the avoidance of the topic by men who hold up the stirrup as a proper type of reinforcement. Some of these arguments have already been referred to in this paper. I believe that a demonstration of the attitude of authorities to

sidestep the question is of equal importance with the demonstration that what arguments have been presented are without merit.

In "Engineering and Contracting," September 10th, 1913, p. 295, Mr. S. E. Thompson says: "The refutation of his claims, which he acknowledges to be contrary to all standard practice, lies in the fact that tests of full size members by such men as Talbot, Turneaure, Johnson, Goodrich, Mörsh, and many others, have shown conclusively that stirrups do greatly increase the strength of the beam, and that the method of design which is now practically standard is safe and conservative. Furthermore, tests show conclusively that stirrups do take tension." This is Mr. Thompson's sole response to a four column article of mine showing that stirrups as placed in the standard way are not to be relied on.

In 1915 I read a paper in Chicago before the American Concrete Institute Convention. This paper criticised the stirrup, the rodded column, and the flat slab co-efficients. It was very fully discussed, and this by such men as Professors Talbot, Hatt, Slater, Lord, Messrs. S. E. Thompson, J. R. Worcester, and other prominent reinforced concrete engineers. All of these men opposed my condemnation of the stirrup. The discussion was long. I answered it all. The American Concrete Institute, Prof. W. K. Hatt, President, has suppressed the publication of that discussion.

At that meeting, in answer to my question, one of the recognised world authorities said that stirrups had performed their function, if found embedded in a beam that had fallen away from its support in a wreck. In all conscience, what is the function of stirrups? And why waste steel, when they could be painted on the side of the beam and perform this function?

A few years ago Prof. Hatt, in a letter to me, said that my arguments against reinforced concrete standards of design had been answered more than once by such authorities as Professor Talbot and Professor Turneaure. The response I made, to which he has never replied, is that neither Prof. Talbot nor Prof. Turneaure nor himself had ever once publicly mentioned my arguments excepting the occasion in 1915, and he was himself instrumental in suppressing the publication of that discussion.

In a beam that is cracked by reason of high shear, of course vertical stirrups will be found to be in tension, otherwise the beam would be almost sure to collapse. Stirrups, if there are enough of them, will delay the failure, or, if stirrups are lavished on the beam, the stirrups will prevent failure. Tests are not needed to demonstrate this, Simple analysis is sufficient. A vast amount of testing already done could have been dispensed with, as it merely tells what analysis would tell, and tell more scientifically and truly, for analysis would go beyond the superficial results of the tests and investigate the permanence or soundness of the beam strength that is contributed by the stirrup and the system of which it forms a part. If, for example, initial failure, as exhibited by the cracked beam, throws the load of the beam on the

horizontal rod in shear or on insufficiently anchored stirrups and then on the concrete between stirrups, as a shearing stress, the system is at fault, and some more rational method of reinforcing a beam for its shear should be sought.

To prove that in a whole beam of concrete of a uniform character there will be little or no stress of any kind in the stirrups, it is only necessary, by analysis, to ask: What possible condition can there be which would produce vertical elongation or vertical shortening in the beam? This question answers itself. If the beam cannot be elongated or shortened vertically, it is quite impossible that the embedded stirrup could be subjected to tension or compression.

It is quite easy to show by analysis that if there are layers of weak or mushy concrete in certain strata of the beam there will be tension or compression in the stirrup, depending on the way in which these layers of "mush" may lie.

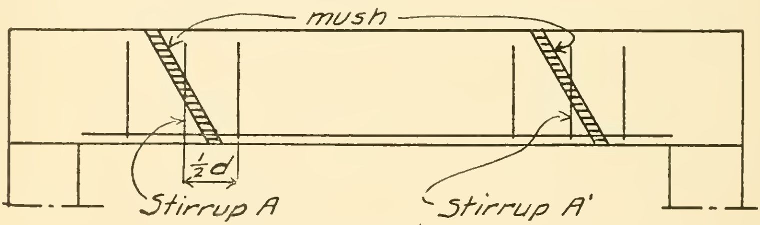


Fig. 8.

Fig. 8 illustrates this. At the left end of the beam stirrup A will be in compression, and it will be compelled to take the full shear of the beam, if the layer of "mush" is inclined as indicated; not half the shear as standard analysis has it. At the right end of the beam stirrup A^1, identically conditioned with A, will be in tension, the full shear of the beam.

Tests have found little stress of any kind, in stirrups in whole beams. Some tests on a building did discover a trifling amount of compression in stirrups. Speaking of these tests I have made the statement "that tests have failed to find any measurable tension in a loaded beam." This statement that tests have failed to find tension, where manifestly tension was sought, has been seized upon with avidity by proponents of the stirrup as being one of the false statements that I am so frequently accused of making. The tests which found a small compression in stirrups and therefore "failed to find tension" are those already referred to in this paper in Bulletin No. 64, University of Illinois Experiment Station.

As compared with the freak tests made on beams where stirrups are so close and numerous that they would give a contractor the nightmare if used in actual construction, the number of tests made on beams where there are no stirrups, but where main tension rods are bent up with a flat bend and anchored, have been few. Some of these have had the rods very imperfectly

anchored and have failed on that account. Some early tests show poor results because of the dry concrete used in those days. Steel is not properly gripped in dry concrete.

It is a surprising fact that there seems to be objection to making tests of this real reinforcement for beam shear. As long ago as 15 years the head of an engineering college asked me for suggestions for tests to be made by his students. I suggested beams reinforced in this manner, and he made the astounding reply that such beams would not be reinforced concrete. In recent years another college asked for a programme of tests, and I outlined one that would go a long way toward demonstrating the value of this type of reinforcement. My programme was so altered as to destroy a large part of its value.

This paper would not be complete without a reference to the structural principles that are at once an explanation for the confidence on the part of designers in short shear members and the very general interpretation by authorities of the results of tests on stirruped beams as warrant for that confidence, for, of course, there must be some reason for a stand that is held to with such tenacity by a whole profession. The following is not to be found in any engineering works on this subject. I submitted a sober and scientific analysis of the thing to the American Society of Civil Engineers recently, and it was returned to me as not being suitable for publication. Doubtless this was because of the clause in the constitution of that Society which forbids the publication of any paper that controverts established fact. This clause is damning to progress and damning to safe design. No man or set of men on earth can lay claim to the omniscience necessary to judge what is and what is not established fact. It is this attitude of professions that makes a profession stagnant, resisting progression, never yielding to it except when compelled, either by external force or internal iconoclasm.

Reinforced concrete authorities have very recently recognised the benefit of end anchorage of main steel reinforcement by allowing twice as much end shear in a beam so conditioned.

It can be shown that a beam is better capable of resisting its own end shear if the main reinforcing rods are anchored over the supports, though there be no shear reinforcement. It can also be shown that this beam shear is not taken by shear in the concrete but by compression, a stress which concrete is well able to take.

Given a simple truss, as in Fig. 9, composed of two top chord members and a tie. Roughly the chord stress is 100,000 lbs. If the top chord has 100 sq. in. of area, the unit stress is about 1,000 lbs. On the 100 sq. in. of cross section of the compression chord the apparent vertical shear in the chord is about 100 lbs. per sq. in., or approximately one-tenth of the intensity of the compression. It is clear that there is not a vertical shear in the chord, but a simple and direct compression. The shear of the truss is taken by the vertical

component of that compression. But suppose this were a concrete beam, the triangular portions being filled out, as in Fig. 10. It could be said that the

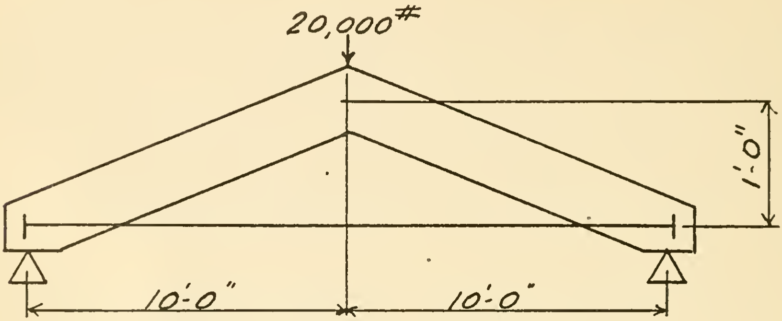


Fig. 9.

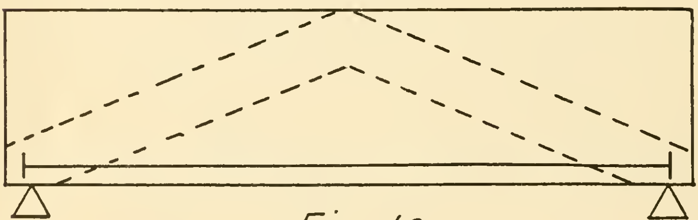


Fig. 10.

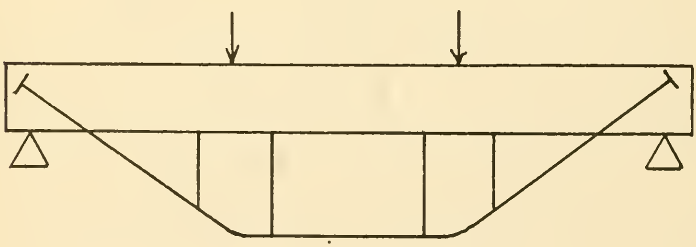


Fig. 11

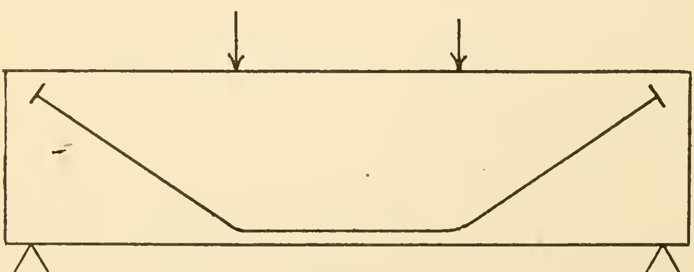


Fig. 12.

beam has no vertical shear provided one thing were true, that is, that the

reinforcing rod had a full anchorage beyond the point of support. It is just here that much false interpretation of tests exists.

Investigators have given little thought to the end anchorage of main rods, either straight ones or bent-up ones, in planning series of tests and in interpreting results of their tests. It has been my observation that wherever there is good anchorage of main rods beyond the point of support of beam, there will be good shear carrying capacity. As an example: In Bulletin No. 29, University of Illinois Experiment Station, p. 63, fourteen beams with deformed bars show average ultimate end shears of 248 lbs. per sq. in., while the average of nineteen beams with plain rods shows 163 lbs. per sq. in. These beams had stirrups. I am not an advocate of deformed bars, but I recognise that a deformed bar will have better anchorage in a very short length than a plain bar. In proper design plain rods will have anchorage enough; and if full anchorage is required in a short length of rod, something better than mere deformity in a bar is needed.

The truss idea may be varied as in Fig. 11. If in this figure the steel rod has full anchorage beyond or at the point of support, there is no shear whatever in the concrete member, not even as a vertical component of direct stress. The whole shear of the truss is taken by the steel rod as a vertical component of the stress in the inclined portion of the rod. By adding concrete to complete the beam, a reinforced concrete beam as in Fig. 12, could be constructed that could be considered to have no shear in the concrete, provided the steel rods were capable of carrying the calculated stress, and provided, of course, that the ends had full anchorage at points over the supports. (It is not attempted here to show the manner of anchorage, but merely to indicate that the rods have some form of full anchorage beyond the points of support.)

Tests have been made and reported that purport to show exceedingly high stress in stirrups, such stresses as 100,000 lbs. per sq. in., stresses that are totally fictitious, and that are based on an imaginary distribution of shear. The fact is, that in a large measure these stirrups were functioning as lateral reinforcement in a compression member inclined a little to the horizontal. For in these laboratory tests the bottom rods always extend beyond the edge of support, and thus approach the condition of the rods of Fig. 10; and besides this, the supports themselves in laboratory tests are generally rigid blocks, which offer resistance to horizontal displacement.

These two features of laboratory tests (partial end anchorage of horizontal rods and horizontal resistance of supports), as causing arch action, have been ignored by experimenters, and hence interpretation of results as pertaining to shear are of little value.

The reasons why end anchorage of the horizontal tension rods is not as beneficial in reinforcing a beam for shear as bending up and anchoring some of the rods, are the following:—

1. In the beam indicated in Fig. 10 the shear of the beam is carried by diagonal compression in concrete. In the one indicated in Fig. 12 the shear of the beam is carried by tension in the steel rod. Tension in a steel rod is more to be depended upon than compression in concrete. Steel is made under supervision; concrete is not always so made.

2. Most beams in buildings are T-beams, and the facilities for anchoring steel rods in the stem of the T are not good; they are not to be compared with those in the upper part of the beam.

3. The inclined compression member of Fig. 10 in a T-beam will be very much constricted near the ends of the span, as only the stem of the T is available.

4. On the other hand, the horizontal compression member of Fig. 12 will be of excellent shape for a compression member, not only because the flange of the T is added but also because the floor slab adds its stiffening effect.

5. In continuous beams which are more common than other types, the bent up rods shown typically in Fig. 12, would in reality extend into the next beam for top tension reinforcement, as shown in Figs. 3 and 4.

It is manifest from the foregoing analysis that bending up rods into the top of beams with a sharp bend, in any of the standard ways of reinforcing for shear, does not constitute an efficient shear reinforcement.

This analysis further makes it clear why a large number of closely spaced stirrups will add shearing strength to a beam, *if the bottom tension is taken care of by rods anchored beyond the edge of support.* The shear of the beam is broken up into zigzag lines of tension in steel rods and compression in nearly vertical lines in the concrete. If the rods are widely spaced, as in standard design, and if the bottom tension is not taken care of by anchored rods, *the beam may break between stirrups and failure may result, even though the design may be in every respect exactly what is required by standard regulations.* Hence:

It is a crime to design a reinforced concrete beam which depends upon stirrups or short shear members to carry the end shear.

SHEAR RESISTANCE.

Reprint from Jour. Inst. Struc. Engrs., April, 1923.

Mr. Edward Godfrey's reply to the discussion on his paper (vide March issue).

It is a matter of gratification to hear of the kindly and tolerant spirit with which my paper on shear reinforcement was received at its reading on February 22 last. It is infinitely better to thank a man for telling you a thing with which you do not agree, and then to turn around and make a straightforward statement as to why you do not agree with him, than it is to suppress the publication of his remarks because you cannot agree with him, and fear the effect of those remarks on others who might be influenced.

I sincerely trust that the Editor will publish my remarks just as I give them. I have had difficulty in unravelling the third-person-singular-and-plural method of speaking, but I trust I shall not herein reverse any gentleman's meaning.

In making my reply, I shall begin with Mr. M. E. Yeatman's remarks. These contain a direct challenge. Mr. Yeatman says that he has always designed beams in accordance with my figures 3 and 4, and he has a quarrel with me because I seem to think that I am one against the world in advocating this type of design.

If he will turn to pages 86 and 87 of the JOURNAL, he will find that in my paper I have commended the Hennebique Patent because it describes this very thing. Also I refer to De Man's patent of the same year, 1898, which shows similar reinforcement; and to the English patent of Hyatt of 1877, showing the same. So that it is clear that the paper does not attempt to set up any claim of originality so far as this type of reinforcement is concerned. Furthermore, on page 87 I have said, "We frequently see designs where engineers have used this type of reinforcement for shear. In this respect designing in reinforced concrete transcends the standard literature of the subject."

Now, if Mr. Yeatman and the rest of my critics will just stand off for a few minutes and look at this thing in an impersonal way, and if Dr. Faber, being the only one of those criticising the paper whose works were referred to in that paper, will listen to what we, in America, call a hypothetical question, I think I can make a lot of things clear that seem to puzzle my critics.

Here is reinforced concrete, an important branch of structural engineering. It has a very voluminous literature, including many standard works. By absorbing all that there is in those standard works, one should be equipped at least to design a reinforced concrete beam safely. Is the seeker after proficiency or correctness, or mere safety in design, not justified in concluding that he has equipped himself to do the latter, at least, when he has faithfully learned all that these standard works have to offer?

Add to these standard works the standards of design that are issued by committees, and associations, and councils, and tell me, should not the disciple who sits at the feet of this array of authorities be supplied with a consistent and conclusive and perfectly safe method of designing a simple and properly reinforced beam? Should he not be supplied with a satisfying analysis of how and why this reinforcement is correct?

Now, if Mr. Yeatman will take down all of this standard literature, to the greater part of which I have already referred, and if he will find one paragraph of text recommending or describing the method of reinforcing beams shown in my Figs. 3 and 4, and stating how that reinforcement is to be proportioned, he will find something that I have been utterly unable to find.

On the other hand, he will find pages and pages of designs illustrated and

described with methods of design detailed, where the sole provision for shear is almost exactly as shown in my Figs. 5, 6 and 7. In fact, variations of these figures comprise the only methods of reinforcing a beam for shear that are described in all standard literature on the subject, admitting the occasional tying of stirrups to the top reinforcing rods as a variation.

It is significant that not one of my critics has attempted to defend the reinforcement shown in my Figs. 5, 6 and 7, nor to show that they are other than standard design. Furthermore, none of my critics has attempted to defend the analysis of stress in stirrups illustrated in my Fig. 1.

It is of no consequence that Dr. Faber is not a strong supporter of the Kahn bar, as he intimates in his discussion (page 100), and it is of no consequence that in some other place besides his book he expressed other opinions on the subject of shear members. As an author, I have never illustrated nor described any standard type of reinforcing a beam for shear for any other purpose, but to condemn it. This, in my judgment, is the only correct method for any author to pursue, who would keep himself free from adverse criticism, namely, to illustrate in his works nothing whatever but what he considers proper design, unless those illustrations are given for the purpose of pointing out their errors. A large part of my book on Steel Designing is taken up with just that kind of constructive criticism. An author is not an editor who can hide behind the alibi that he is merely illustrating current practice. He is supposed to be a guide pointing out the way to correct design.

If it is possible for me to design a structure that does not violate a single sentence in all of the standard literature on this subject, and does follow the examples set by all of the standard writers, and if it is possible for that standard structure to collapse in ruins, it is hardly necessary to say that there is something radically wrong with the standards.

I could fill a whole issue of the Journal of this Institute with detailed description of wrecks of structures that have been pronounced standard and correct by engineers. I could point to an almost endless number of beams that have had alleged reinforcement for shear, of the standard type illustrated in my Figs. 5, 6 and 7, which have broken away from their supports, failed in shear, just as those figures show they are apt to break, not carefully nursed laboratory beams, but beams in actual structures, made of well-seasoned concrete, many of them.

One reason why these building tests are of infinitely greater value than laboratory tests is because in the building there is a heavy longitudinal shrinkage tension in the beam that could not exist in the isolated specimen.

Acres and acres of structural floors have collapsed, and none of this wreckage could possibly have happened if properly reinforced columns and proper shear reinforcement in the beams had been used.

If the statements of the foregoing three paragraphs are true, I submit that

the fault lies with the watchmen who are stationed in the tower of authority, and who have failed to blow the warning trumpet.

On page 101 Dr. Faber refers to alleged or supposed inaccuracies in my statements of the expressed opinions of the authorities to whom I referred. He has exactly the same opportunity that I had of learning those opinions, for the things I quoted are published statements by these men. If they have private views contrary to these statements, it is time they were making the same public and repudiating their published utterances, and this paper and discussion are, therefore, very opportune. We have had quite enough wrecks due to designs that do not supply proper shear reinforcement in beams and properly reinforced columns.

The private views of the men I have mentioned, and the method of design that they might use in their private practice, as well as the method of design that any man discussing my paper might use, have no bearing whatever on the matter of the standards of design of the engineering profession. If those standards, as described in the standard literature on the subject, are not safe to follow, it is time that literature were relegated to a museum as exhibiting the struggles of men to reach a knowledge of safe design in a new branch of engineering.

Dr. Faber says, on page 99, that it is only in bad designs that the stirrups, acting as truss members, lack the vital essential of end connections. Also he says that the stirrups should be adequately connected to the compression member. His figure, on page 102, is presumably reinforced in the proper way. It is assumed that the stirrups have large hooks at the upper end that are in a plane at right angles to the paper. Now, strip this figure of the large hooks on the ends of the horizontal tension rod, which are not required by any standard and form no part of standard shear reinforcement, and his figure will be identical in every essential with my Fig. 5. The lines of possible breaks can be drawn to dodge every vestige of steel reinforcement.

What assurance does it give of safety to anchor the stirrups into the compression flange, when that compression flange, as part of the beam, can be precipitated, with the rest of the beam, into the basement of the building? Why waste steel merely to hold the parts of a beam together when the beam itself is not held to the support by anything more reliable than tension or shear in plain concrete?

Dr. Faber's figure on page 102 is doubtless a response to my demand for a satisfying analysis of the stresses in a stirruped beam. The lines representing compression members in his Howe truss are rather lines of most probable cleavage in the concrete. At the upper end these lines point to the very tip of the stirrup. The stress in the stirrup and in the concrete compression member would be wholly eccentric—a shear stress in the concrete. Regardless of the spacing of these stirrups, every stirrup would have to take the full beam shear of the system. Suppose a beam had a section 8 in. × 20 in., and a

shear of 600 lb. per sq. in. (allowed by L.C.C. Regulations. See Faber's book, Vol. II., Three times one-tenth of compression value). The shear of the beam is 96,000 lb., and the poor stirrup would have to take all of this. Is this not an awful load to hang on an unsuspecting tension rod? Surely something is wrong here, and it is not with my destructive criticism nor that of Dr. Faber in his comment, Vol. II., p. 244, on the L.C.C. Regulations.

Shear reinforcement must carry the shear of a beam into the supports. Some positive reinforcement to carry the shear of a heavily loaded beam into the supports is essential, and this the stirrup does not supply, for the simple reason that the stirrups do not reach the support, and beams can, and do, utterly fail because they shear off close to the supports, as illustrated in my Figs. 5, 6 and 7.

On page 100 Dr. Faber states that toward the end of my paper I show diagrams and arguments based entirely on the truss idea. He refers, of course, to my Fig. 12, and because this may be analysed as a truss, he seems to imply that it is no better conditioned than Figs. 5, 6 and 7, which are commonly analysed as trusses. It is of utmost importance to point out the essential and vital difference between the truss analysis that justifies Fig. 12 and the pseudo-truss analysis that attempts to justify the standard reinforcement of Figs. 5, 6 and 7.

Fig. 12 is a diagrammatic representation (as the text clearly states) of what is shown in a practical design in Figs. 3 and 4. Main reinforcing rods are bent up and fully anchored beyond the edge of the support. It is utterly impossible for this type of beam to fall away from the support without completely severing the bent-up rod or pulling it out of its anchorge. This is real shear reinforcement. Furthermore, this supposed truss would not, by any stretch of terms, be called a Howe, Pratt or Warren truss, which are the truss analyses specifically condemned in my paper.

Figs. 5, 6 and 7 are typical representations of all the standard systems of shear reinforcement described in all the standard works on the subject. Any or all of the alleged shear members could be imagined to have hook anchors spread laterally into the concrete—the figure would not be altered in the least. If any one of these beams should be cracked on the lines indicated, its usefulness would be at an end, and the steel would not be in the slightest manner disturbed, though that steel is alleged to be reinforcing the beam against that very failure.

If Dr. Faber, and all of the rest of my critics, cannot see the enormous significance in the difference between these two ways of analysing as a truss and of reinforcing for shear, and if they still think, as Dr. Faber says on page 99, that I have simply made a very large number of statements, and that it is very difficult to make out clearly where I differ from orthodox practice, I cannot see where it is my fault.

The difference is between the undisputed safety of Figs. 3 and 4, which

are not standard nor orthodox, and would not be permitted (without stirrups) by any standard work or regulation, and Figs. 5, 6 and 7, which are standard and orthodox, and whose dangerous character is clearly demonstrated in these figures. In a million-dollar building which I re-designed I cut out many tons of stirrups that were shown by the original designer, and not one stirrup was used in the entire structure.

Dr. Faber, on page 103, says that shear members inclined at small angles are extremely good, provided their ends are adequately hooked. This is exactly my Figs. 3 and 4, and exactly the thing that I have been advocating against the entire engineering profession since 1906, and exactly the thing that no standard work or specification ever written would allow or recommend a designer to use, apart from those allowances which, as my paper states, I forced committees to incorporate in their reports.

Dr. Faber says that shear reinforcement must carry all of the shear, and not that which the concrete cannot carry. Mr. Ewart S. Andrew says that all British designers agree to the same thing, and he has never met one whose opinion differed. I introduce him to a British engineer named Dunn, who published a series of lectures, in which he says what I have quoted on page 82, which is a direct contradiction of Mr. Andrews' statement as to British practice. I introduce him also to the framers of your R.I.B.A. Regulations, which, as quoted in Dr. Faber's book, Vol. I., 1919, p. 316, allows this same practice.

Mr. G. A. Gardner questions the constructive character of my paper. In America sometimes a whole block of large buildings is utterly wiped out in order to put up one building of modern type, maybe of 40 or 50 stories. Everything in those eliminated buildings is rejected. This is a destructive kind of work for a while, but the final result of it all is certainly constructive. Reinforced concrete practice needs some such constructive work. Mr. Gardner says that the paper would have been greatly enhanced if I had mentioned the various tests, supplemented by necessary data and measurements. What measurement would be given where a beam fell from its position in a floor to the basement, simply because there was no steel whatever used to tie it to the support? What measurements would be given in two building tests made by me, one where reinforcement was as in Figs. 3 and 4, and absolutely no cracks could be found, and one where reinforcement was by stirrups, and scores of wide, open cracks were plainly visible? What other measurements are needed in similar comparable tests made on a building by Prof. Talbot (page 88), where he failed to find any cracks in beams of the type of Figs. 3 and 4, but found good high tension in the inclined rods, proving that they were functioning, and where he found cracks in beams reinforced as in Fig. 7, and found compression in the alleged shear reinforcement, showing that it was not functioning? What measurement would enhance the lesson of a test where one-half was reinforced with stirrups, and the other half as

my Figs. 3 and 4 (though the bent-up rod was not fully anchored), and where, under symmetrical load, the cracks and the failure occurred on the stirruped side? (See page 87.)

Mr. C. Spencer Payne, on page 107, says the vertical loops (stirrups, I suppose) must take compression. Dr. Faber says they must take tension. So where are we?

Mr. W. A. Green's statements about the members of a steel Pratt truss being in the same case as stirrups, in that there is nothing pulling the top boom away from the bottom, is not borne out in fact. Because of complete articulation every tension member in a steel truss is elongated, and every compression member is shortened under stress. Numerous tests have proved this. This cannot happen in the case of the alleged tension stirrups of a reinforced concrete beam. Articulation is not possible, and it is only after the beam cracks that the stirrup can receive any tension.

I have sent copies of the Journal to American engineers whose works and utterances are criticised, with the invitation to reply. It may be weeks before I hear from these men, but in the meantime I shall send the foregoing response to my British critics, as I have the assurance of the Editor that space will be given for this reply.

I wish to assure my fellow-members of the Institution that my sole aim is to reach a sound basis of design in reinforced concrete. I have in preparation a paper on reinforced concrete columns which I should like to beg the indulgence of the Institution to have read at a future meeting. This paper takes exception to standard methods of designing columns, but it also points out the fact that British practice is superior to American practice in this particular.

AUTHOR'S CLOSURE

Reprint from Jour. Inst. Struc. Engrs., June, 1923.

I have already answered my British critics who commented on the paper read at the meeting of the Institution last February. On receipt of copies of the "Journal" I sent a copy to each of 20 American engineers and authorities, and a letter inviting these men to discuss the paper and make response to the adverse criticism which my paper contained on their works or utterances. Twelve of these men vouchsafed no recognition of the letter or of receipt of the complimentary copy of the "Journal." Three of the men acknowledged receipt through their secretaries. Two acknowledged receipt personally, without comment.

Prof. Geo. A. Hool said "I haven't had the time to any more than glance over your paper on Shear Reinforcement, but that glance was sufficient for me to decide that I have no desire to make any comments. Nothing short of

a complete discussion would be of any account, and I have not the time or inclination to undertake such a task."

Mr. John S. Sewell replied thus:—

"You have taken a single sentence from a discussion which I submitted on one of your papers in 1910. This sentence, taken by itself, together with the reference to your figure 6 on page 91 of the 'Journal' of the Institution of Structural Engineers, produces a wholly wrong impression as to what I really said in the discussion in 1910. Moreover, your figure 6 is not a fair representation of the corresponding figure in your paper of 1910.

"To be perfectly frank, I am unable to reach a definite conclusion as to what you really mean to say in your reference to me in the copy of the 'Journal' of the Institution of Structural Engineers. I am not surprised that some of the English commentators have some difficulty in divining what you are really driving at. However, the essence of the proposition, so far as I am concerned, is that the manner in which you have handled the single sentence which you quote from what I said amounts to garbling the record.

"I suspect that other American engineers whom you have quoted would feel as I do. Under the circumstances, I consider any extended reply to what you say as entirely unnecessary."

Mr. C. A. P. Turner responded with a contribution to the discussion which, I presume, the Institution will publish.

The foregoing do not really need any response. Sometimes a man receives the strongest confirmation of his ideas from those who reject and oppose those ideas. But when men affect to be unable to see "what I am driving at," namely, that a stirrup which does not reach a support cannot tie a beam to that support, didactic methods are necessary.

I shall first comment on the general character of the response to my appeal to the men concerned, either to defend or repudiate their utterances.

In my paper I made the clear charge that the men who are responsible for standard methods of design in reinforced concrete are perpetrating an engineering crime, for the reason that beams can and actually do fail, though in their design not one syllable of standard regulations is violated, and nothing is omitted that any standard work requires to reinforce the beams.

Whether my charge is true or untrue, silent contempt is not an adequate response. Pressure of business is no excuse for failure to defend one's published statements or to repudiate them, if no defence can be made. Only two honourable courses are open—one is defence, in the nature of demonstration of correctness; the other is repudiation.

If the works and regulations for which these men are responsible clearly require that a beam have steel rods tying it to the support in such manner that the steel rod must sever or pull out of full anchorage before the beam can be separated from its support, it is assuredly possible to demonstrate this. If the said works and regulations do not require this simple and abso-

lutely necessary reinforcement to prevent failure, the men responsible therefor are guilty of contributing to the growing list of failures, and must be held so until they revise and repudiate their published recommendations.

Replying briefly to the few who have made comments:—

Mr. Sewell's contentions are utterly absurd and groundless. If he said anything else worth quoting in 1910, here is his opportunity to quote it. If my figures of 1910 and 1923 are not similar here is his opportunity to demonstrate it. (See Trans. American Society of Civil Engineers, Vol. LXX., 1910, page 55, for the 1910 figure. The engraver made the shear member a trifle longer than my sketch.) I repeat it, they are in every essential similar.

Mr. Turner blows hot and cold. He commends my stand and then says I am wholly in error. He says that the inclination of bent-up bars should in general not be greater than about 30 degrees; then he appears to class vertical stirrups (90 degrees inclination), in next to his last paragraph, as both efficient and useless.

His contention that it is deep beams that need provision against diagonal tension failure is not exactly new. I pointed this out in "Engineering News," July 12, 1906, page 30. But my conclusion was arrived at from a different premise altogether.

His statement that continuous or restrained beams do not fail by diagonal tension (shear) at the supports is contradicted by tests made by Withey, and described in Bulletin No. 175, University of Wisconsin. This bulletin describes a number of tests made on continuous or restrained beams, and many of these failed by shear or diagonal tension right at the support. Furthermore, the beams had stirrups and other short shear members, the standard method of reinforcing for shear. None of them had what I would consider the proper type of reinforcement for shear.

Mr. Turner does not intimate how he would reinforce a beam to take care of the shear. Furthermore, it is to be noted that neither Mr. Turner, the only American engineer who has ventured to enter this discussion, nor any of my British critics has attempted either of the following tasks:—

First: To show that my Figs. 5, 6 and 7 are not the only methods required by standard authors and regulations for reinforcing a beam for shear.

Second: To defend these methods of reinforcing for shear.

Here is the crux of the matter. Whatever difficulty any reader may have in "divining" what I am aiming at, let him simply take these figures and ask himself these questions:—

Do these figures show standard reinforcement for shear? Is there any tendency in beams of this type to break as indicated? Does the reinforcement shown in any manner whatever hinder that failure?

Prof. A. N. Talbot, in the discussion of my paper in 1915 (which the American Concrete Institute officers refused to publish) said: "The beam

ought to be tied into the wall, tied in the support." Why, then, did he not write this into the Joint Committee Report, for the Design portion of which he was responsible? This is one of the most important statements ever made by this eminent authority. Nowhere else can I find that any standard authority ever said such thing, and yet the American Concrete Institute refused to publish this statement.

A beam is not tied into a support properly nor economically when a few senseless stirrups are looped over a horizontal rod. Stresses in steel will not turn sharp corners in this manner. No analysis ever attempted to show how a vertical tension in a stirrup is translated into a horizontal tension in a top horizontal rod.

It seems to me that the case against the stirrup has been amply proven. The only thing left to do is to arrange for the funeral ceremonies of the stirrup and the whole family of short shear members. We can design safely and economically and properly without any of them.

To those who object to my method of speaking let me say that it is time for irony and sarcasm. I started out in 1906 in a series of articles in "Engineering News" to question in the mildest, soberest manner the value of the short shear member. I have repeatedly challenged the entire engineering profession to put forth a sane defence of the short shear member, as required in standard works and regulations. The most they have done is to assail me and my "private views." Wrecks have gone on apace. Here is a spectacle of 20 eminent authorities directly challenged to discuss a thing, and the only one who responds does not touch on the thing assailed, namely, standard design of beams.

CHAPTER V

FAILURES OF REINFORCED CONCRETE STRUCTURES

An Open Letter to W. A. Slater

By Edward Godfrey

(Reprinted from *Concrete,* Detroit, February, 1921)

Dear Mr. Slater:

I have your letter asking for a list of building failures—failures, as I understand it, where the concrete has had time to harden. You state that the purpose of asking for the information about these failures is to enable you to judge whether the cause of failures is lack of opportunity for the concrete to harden, or whether in some cases it is possible that the working stresses used in design are too high for the concrete used, even though it has had ample opportunity to harden.

You seem to want to know only about cases where the concrete has seasoned. Strict adherence to this limitation would probably eliminate some of the cases that I shall cite, in part at least, for there may be parts where the concrete was still unhardened or maybe frozen. If it could be shown that every part that collapsed was green concrete, I would be willing myself to eliminate those cases and to forget them. But here is the crux of the matter. If a little green or frozen concrete, by its failure, pulls down with it acres of seasoned concrete, that, in the nature of the case, must have had weeks or months to harden, the green concrete is merely the tripping cause, and there is something radically wrong somewhere else. It is unthinkable that reinforced concrete construction, or any other kind of construction, is of such fragile nature that a small spot of defective workmanship can be cause for a perfectly general and horribly serious and total collapse.

Furthermore, the length of time concrete should harden before it is fit to stand up depends upon the intensity of the stress thrown upon it by the removal of the forms. For example, I have seen forms removed from hooped columns poured in winter after allowing only two days to set. This is poor practice, but nevertheless the columns exhibited no sign of distress whatever. It is on record that interested persons attributed a failure in August to removal of column forms in ten days, and set up the claim of cold weather; also an interested person argued publicly that the reason the Bell St. Warehouse test was a failure was because concrete 116 days old was not seasoned.

You will pardon my interjection into this thing of some remarks. Let me

comment on the quotation from your letter in the first paragraph of this letter. It is prejudgment and it is narrow. There are other things besides unit stresses and hardened concrete. There is such thing as design, and design that is essentially and irrevocably faulty. You seem to exclude design —type of design—entirely from your purview. Unit stress does not cover all there is in design. It is in fact only incidental. There are types of design that are not compensated for in any degree by alteration of unit stresses. A rope of sand is bad and dangerous, not because the unit stress is too high. A rope of concrete is precisely the same; it is wrong because it is a rope. Any tension member of concrete is unsafe, regardless of the unit stress. A rodded column is bad, not because the unit stress is too high, but because it is analogous, in some degree, to the concrete rope. The only case on record of rodded columns made like they are in a building and tested, stood only 400 lbs. per. sq. in. ultimate load. One case that I shall cite, broke in a building at 200 lbs. per sq. in. This was a thoroughly seasoned column. Others broke at loads near this amount. The reason the concrete tension member is bad is because the slightest start of a crack means utter failure. The reason a rodded column is bad is because it contains in itself the elements of its own possible destruction, namely, rods in compression, due to concrete shrinkage, pushing out on brittle concrete and tending to spall it off; and because this spalling increases tremendously the strain on the rest of the column.

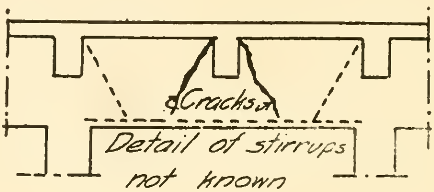
Stirrups come in the same category. No matter how heavy a vertical stirrup may be that is set one-half of *d* away from the end of the beam, it will not affect in the slightest degree the tendency of the beam to break off in that *half d* zone where the maximum shear occurs, as many, many beams in wrecks have done.

Permit me to say, too, that I do not profess to have anything like a complete list of failures. I simply keep account of what I hear about. In no great measure are folks carrying this information to me. On the contrary, I have more than once been ordered off the premises. Besides this, information regarding failures of buildings already occupied is the most difficult to obtain; for obvious reasons owners do not want it known. Most of what I have came to me by accident.

One building, that I shall call No. 1, suffered entire collapse of the roof and supporting columns. The columns in the top story were rodded, those below the top floor were hooped. Frozen concrete probably had much to do with this, but why did the collapse stop where the rodded columns stopped? Why did the builder rebuild with hooped columns in this top story? I was employed to test the floors that did not collapse. The design was a beam and slab type, with stirrups in the beams, but no bent-up-and-anchored rods. Tests of the designers' bare safe load produced frightful conditions, particularly in the matter of large deflections and wide open shear or diagonal tension cracks. Reinforcement by adding new girders was necessary. About

10 or 12 years later the building was again in need of strengthening because of original weakness in the type of design. This building is described in more than one standard work for the guidance of students.

Failure No. 2 is a beam and slab type of design with stirrups and bent-up-but-not-anchored rods. It was occupied for some time and large open cracks developed in the girders, as shown in the following illustration.



I interviewed one engineer and had access to the report of another engineer who investigated this failure. They stated that the load on the floors was only what was intended by the designer to be placed on them. The excellent photographs showed no tension cracks in the bottom of the beam, but great open diagonal tension cracks in the concrete web.

Failure No. 3 is a flat slab design, a large warehouse that was in use for some time. The floors cracked up and sagged about 3 in. Incidentally the *interested* engineer recommended that these hollow spaces be filled up with concrete. This is one case where unit stress tells the story, but the type of design, even here, is inseparably associated with moment coefficients that go with it.

Failure No. 4, Bridgeman building, Philadelphia—*Concrete Engineering*, July 15, 1907, p. 26; other papers, I believe, described this failure about the same time. I do not know how long the concrete had hardened, and do not care in the slightest degree. There were rodded columns, 9 in. square and 19 ft. 6 in. long. There were stirrups in the beams, and there were bent-up-and-not-anchored rods, as stated by Mr. H. F. Porter. Mr. Porter, an eye witness of this failure, and a concrete engineer, states in *Trans. Am. Soc. C. E.*, Vol. LXX, p. 118: "The cracks in the beams, due to the dead weight alone, were most interesting and illuminative of the action which takes place in a concrete beam. They were in every case on the diagonal, at an angle of approximately 45°, and extended upward and outward from the edge of support to the bottom side of the slab. Never was the necessity for diagonal steel crossing the plane of weakness more emphatically demonstrated." This shows the absurdity of the Joint Committee and American Concrete Institute Reports, which say that beams do not break this way. Mr. Porter says further, in regard to the absence of anchoring steel across the supports: "The result was that the ties between the rib and the slab and across the support being lacking, some of the beams, the forms of which had been removed

prematurely, cracked of their own dead weight, and later, when the roof collapsed, owing to deficient bracing of the centers, it carried with it each of the four floors to the basement, the beams giving away abruptly, over the supports. Had adequate tie of steel been provided across the supports, the collapse undoubtedly would have stopped at the fourth floor."

The foregoing quotations, please observe, are words of a man who was on that occasion severely criticising me for my stand against vertical stirrups as a means of carrying the end shear of a beam or its diagonal tension. Please read his words again with this thought in mind.

Failure No. 5 is a building at Vancouver, B. C., described in *Engineering News,* August 14, 1913, p. 290. This building had bars bent up and a small hook over the support, but the hooks were several inches from meeting. A portion of the building collapsed. Girders pulled out of the columns. There were stirrups in the beams, said to have been misplaced. In all conscience, what effect could a bushel of stirrups have had on these beams when they broke off, where no standard in existence requires a stirrup to be placed? What office could stirrups have performed in this building? They did not prevent the failure of the beams in shear or diagonal tension any more than if they had not been present, and this is the only possible office that shear reinforcement can be expected to perform. They would be exactly as effective to prevent a wreck of this sort if they had been painted on the sides of the beams. Mr. A. P. Hueckel, who examined this collapse, stated that the principles of good design were not applied. "The reinforced concrete frame was made up of poorly hooped columns, carrying beams." He also says that the initial failure was at the junction of a beam with a column, due to insufficient connection there.

Failure No. 6 is the Youngstown Hippodrome. This is described in *Engineering and Contracting,* December 2, 1914. I visited this wreck and photographed it just after the collapse, and was driven off it. It was a plain case of bad and standard design. Beams were not anchored to walls and columns and they fell away by breaking off close to the edge of supports, exactly where toy beams are said never to break, by the standard writers. The breaks were clean breaks through concrete.

This building had rodded columns as well as unanchored beams, and the columns broke up in chunks. A large part of the building collapsed into the basement.

Failure No. 7 is a reservoir roof in Madrid, which failed about 20 years ago. I have not the reference where the construction was described. It is one of the classic failures. It was a rodded column structure. The columns simply dropped their load.

Failure No. 8 is a United States Custom House at LaCeiba, Honduras, C. A., described in *Engineering News,* October 29, 1914. "The main structural elements in the design were square columns 12″ x 12″ in section, rein-

forced with corner rods and spaced on the corners of the bays 20′ x 21′ 3½″. The thing collapsed.

Failure No. 9 is a reservoir roof at Annapolis, described in *Engineering Record,* March 13, 1909, p. 291. This roof was carried by columns 9″ x 11″, "reinforced with four ⅞″ Johnson bars placed 1″ from the surface and banded every 12″ with ¼″ bars. The columns are 9′5″ high from the floor to the under side of the beams." This construction meets the requirements of standard practice. Hear this quotation from *Engineering Record:* "One interesting feature of the failure demonstrates both the remarkable vitality of good concrete construction and the excellent character of the workmanship on this job. Column M was broken by the impact of the falling roof, and in falling pulled the reinforcement from the bottom of beam N, leaving the roof slab intact with the upper part of the beam hanging from it. Fig. 5 shows the head of the column supported by the reinforcement stripped from the under side of the beam for more than three-fourths of its length. The roof slab, however, did not give way, carrying over two bays of the load it was supposed to carry over one. The prop shown in Fig. 5 was placed under the slab and a portion of the load was removed. When this was done, the slab recovered about ½″, leaving the prop free." The above quotation is sufficient information as to the *seasoned* character of the concrete and its quality. It is absolute proof that the rodded columns were weak and the sole cause of the wreck. A test was being conducted on this roof, but the portion that collapsed was five or six times as large as the tested portion.

Failure No. 10 is the Spang-Chalfant building, near Pittsburgh. This is described in *Engineering News,* February 29 and March 14, 1912. I saw and photographed this building the day of the failure and made a complete study of the plans. There was some frozen concrete, but it was nearly, if not quite fully, supported by forms. There were cantilever slabs from the side of wall girders, and a single pile under a column having a dead load of 140 tons. Settlement of this column was observed before the wreck, and this settlement started the breaking up of the brittle dish-like rodded columns. Talk about unit stresses—one corner column was about 24 in. square. Figure what load that ought to take. It did not even stand upright. The beams had no steel anchored into the supports, and they broke off close to the supports.

One column in this building, several bays from the wrecked portion, broke clear through at the time of the wreck, from the shock. The unit stress on this rodded column was 200 lbs. per sq. in. This is only a fraction of the crushing load of this column as it would be exhibited in a laboratory test, the sole criterion of the standard writer. Hundreds of rodded columns have failed in wrecks at loads a fraction of their crushing loads, as given out by laboratories, and even at a fraction of their alleged safe load.

A test was made on the floor of this building, in an effort to establish the

fact that frozen concrete, and not the design, was to blame. The test was on a portion of the floor deeper in the slab and more heavily reinforced than the parts that collapsed. It was an interior bay, supported on all four sides with girders, and thus in every respect very much stronger than any part that collapsed.

Failure No. 11 is the Stark-Lyman building, at Cedar Rapids, Iowa. This is described in *Engineering Record,* November 22 and November 29, 1913, and January 2, 1915; also in *Engineering News,* November 27, 1913, and January 8, 1914. The girders had rods bent up with a sharp angle away out from the support, and not anchored. The columns were hooped for three stories above the basement and rodded the other four. I fancy that the exterior columns were rodded and that the hooping of the interior columns did not go through the floors, though this is not stated in the information available. The columns broke into story lengths. In the 40 ft. of the building that collapsed, the seven story columns were broken into short lengths, lying in the ruins. One exterior column, in the portion of the building that stood up, sheared off at an angle of approximately 30° to the horizontal, just below the second floor. (*Engineering Record,* January 2, 1915, p. 31.) This action of a thoroughly seasoned column is absolute proof that columns do fail by diagonal shear under only a fraction of their ultimate load, though Professor Talbot says that concrete does not fail *by shearing and spalling out in chunks. Such condition comes after the maximum load has been reached.*

Of the concrete in the Stark-Lyman building, Mr. C. A. P. Turner says: "The concrete was of unusually good quality." (*Engineering News,* January 8, 1914.) Mr. Turner states that in his judgment the point of initial failure was in one of the rodded columns. He also points out the relative weakness and unreliability of rodded columns and the toughness and reliability of hooped columns, even where the concrete is not thoroughly cured.

Failure No. 12 is the Bixby Hotel at Long Beach, Cal., described in *Engineering News,* November 29, 1906. This building had rodded columns; some of them were round and had a number of round upright rods embedded in them. This was not a case where standard provision only was made for shear. There were extra Kahn bars across supports. The effect of this improvement on standard design is seen in a number of stumps of girders left projecting out from the columns. Mr. H. Hawgood, in *Engineering News,* Vol. LVI, p. 556, says: "The foregoing would appear to eliminate from the direct cause of the accident, the materials, the foundation and the floor system, leaving the responsibility either upon the finished permanent columns, or upon the temporary wooden posts supporting the roof construction, or upon both." A large part of the building collapsed. If the girders had not been anchored into and across the columns, doubtless much more of the building would have been dragged down by whatever started the collapse. The girders

would have pulled out chunks of the columns with them and so weakened these as to cause failure.

Failure No. 13 is the Kodak building, in Rochester, N. Y., described in *Engineering Record,* January 5, 1907, and February 2, 1907, and in *Engineering News,* January 3, 1907. A large section of the building collapsed. The columns were rodded. The girders, many of them at least, had no anchoring steel into the supports. A sketch in *Engineering News,* January 3, 1907, shows how columns and girders fell apart in the failure. This collapse happened in November, which is not a winter month, and on the testimony of Mr. Thacher, the concrete where the failure started was three weeks old. On the same authority, the columns that failed were subject to only 350 lbs. per sq. in. of compression.

A block of wood in column 37 looms large in the report of this failure, and yet the testimony of experts is that it started in column 47. Thus insignificant and irrelevant trifles are brought in to cloud the issue and divert attention from inherently bad design—standard design.

Failure No. 14 is the Hencke building, in Cleveland, O. See *Engineering News,* December 8, 1910, and January 26, 1911, and *Concrete Engineering,* December, 1910. This large four story building collapsed into utter ruin just a short time before it was to be occupied as a department store. This was another case of rodded columns and beams not anchored to the supports. Hardly a stump of a column remained vertical. The so-called reinforcing rods broke out of the columns and curled up in every shape. A little dirt was found in the concrete. You can guess what the report was. I guessed it several months before it came out, as you will see by reference to *Engineering News,* January 26, 1911. Unit stress had absolutely nothing to do with it. It was simply another case of wretched and standard type of design.

Failure No. 15 is the Alameda Hotel, at Kansas City. This is described in *Engineering Record,* October 5, 1912, and November 2, 1912. I quote from *Engineering Record,* pp. 500 and 501: "The interior concrete frame work was of standard column and girder design. The floor slabs were of Kahn concrete—hollow tile construction. The tiles were 8″ deep and 12″ x 12″ in plan, spaced 16″ on centers. A $\frac{3}{4}''$ x $2\frac{3}{16}''$ Kahn bar, with cross sectional area of 0.79 sq. in., was placed in each 4-in. trough between the tiles. The troughs were filled with concrete and 2″ of concrete over the tile." The span was about 22 ft. There were rodded columns and no anchorage of steel into supports in this building. The lack of anchorage exhibited itself by a great slab of beams flopping down like a big cellar door and leaving the wall. It is said that there was a load of cinders on the roof—the account states that the roof was designed to carry these cinders.

Failure No. 16 is a building in Detroit, described in *Engineering News,* January 9, 1913; *Engineering Record,* November 30, 1912; and *Concrete-Cement Age,* December, 1912. In a three-story reinforced concrete garage

about four bays dropped. The design was the usual rodded column standard, with beam steel not anchored into the supports—or, to quote: "The layout was of the ordinary beam-and-column type, carrying tile-and-concrete floors of varying spans. The columns were rectangular in section, reinforced with standard Gabriel ovoid bars. In most of the breaks between the beams and columns there was no evidence of any reinforcing connection between the two members, other than an inch or two intrusion of the beam rods into the column. These pulled out, leaving holes in the columns." (*Engineering News,* pp. 86, 87.)

Failure No. 17 is the Fairbanks-Morse building in Winnipeg, Man., described in *Engineering News,* October 5, 1911. Nine bays of the roof of a reinforced concrete building fell. This was a Kahn design, with rodded columns. The columns had ties spaced 12″ apart. In the wreck the columns broke up into chunks, and the rods curled up in the customary fashion. As to the character of the concrete, I quote: "There is no evidence of a skimping of cement or of anything but the best practice on the part of the contractor." I quote again from *Engineering News,* October 5, 1911: "It will be noted that the fractured ends of these columns (referring to photograph), show the outlines of typical crushing, and that the reinforcing steel which projects from the columns is curled down and inward, as would be the case with this kind of failure. It would seem fairly evident that the columns were the first to fail. With the shores removed, each column was called upon to bear about 22,000 lbs., or a trifle over 150 lbs. per sq. in."

The concrete of the columns was *1:2:3 mixture and it had set ten days in August, and they failed under 150 lbs. per sq. in. Unit stress—bosh!*

Failure No. 18 is a small girder bridge that failed after being in use for some time. See *Engineering News*, December 27, 1906. This fell under the load of a traction engine. It was a Kahn design, lacking anchorage into supports, a design of a standard type.

Failure No. 19 is also a small girder bridge that failed after being in use for some time. See *Engineering News*, April 23, 1908. The style of design was the same as No. 18. This bridge *fell with no load upon it, two years after it was built.*

Failure No. 20 is a small theatre in Cincinnati, shown in *Engineering News*, December 26, 1912. The faults were numerous. Columns were rodded, girders appear to have been light, rods were bunched in the bottom of the girders. The thing collapsed in ruins. The rodded columns nearly all broke up. Some of the girders seemed to crumple in the middle, indicating a weakness in the compression side.

Failure No. 21 is the Bell St. Warehouse in Seattle, a mushroom flat slab building. This building did not collapse, but showed excessive weakness, amounting to failure, under a test load, particularly in the steel, where

stresses were measured as high as 44,000 lbs. per sq. in. The concrete had set 116 days. The reports claimed that rods were not placed high enough in the slabs, but Mr. Turner did not make any such claim. He said the concrete had not had time to cure properly.

This test is described in *Engineering Record,* May 13, 1916, and more fully in an issue of *Proc. Pacific Northwest Society of Engineers.* The slabs cracked badly and outside columns cracked badly. Measured stresses in concrete were as high as 2,930 lbs. per sq. in. The test load was a little more than the supposed safe load.

I interviewed personally one of the engineers engaged on this test. He said they were afraid the floor was going to collapse. The owners would not allow a row of the outside bays to be tested to their normal safe load, for fear of collapse.

Failure No. 22 is the Edison building. See *Engineering News,* Vol. 72, p. 1234, and *Journal American Concrete Institute,* August, 1915.

Parts of this building collapsed during the fire. Many of the rodded columns spalled badly in rooms where the heat was not sufficient to destroy insulation on electric wires strung through the ceiling. A number of columns failed and dropped the floors. There is no doubt that hooped columns would have stood up in this building and carried the floors in spite of the fire.

Failure No. 23 is a tobacco warehouse in Norfolk, Va., which recently collapsed during a fire which destroyed the contents. The Fire Underwriters have recently issued a bulletin describing this fire. The photographs indicate that the columns were rodded and the girders had bent up but not anchored rods. No one with a knowledge of the properties of concrete can doubt that properly hooped columns and anchored beams would have preserved this building intact with only superficial or local damage.

A significant fact might be interjected here concerning fire and seasoned concrete. In *Concrete Engineering,* February, 1912, p. 209, there is a photograph of a Turner flat slab building with round, and presumably hooped, columns which had set for eight days in winter in Canada. A fire destroyed the whole timbering. The concrete was damaged only by a slight surface spalling.

Failure No. 24 is a warehouse at Far Rockaway, N. Y., described in *Engineering Record,* January 20, 1917, p. 98. Here rodded columns and beams, with so-called shear members failed in a fire. Hooped columns and real shear reinforcement would have saved the structure.

Failure No. 25 is the Havre Holding Co. building, described in *Engineering News,* November 30, 1916. This was a flat slab roof, the concrete of which had set from one to two weeks, in November, when the temperature ranged as high as 60°. The columns were round and rodded. They all crushed but three, and let down almost the entire roof. Exterior supports were brick

walls. The faults in type of design were rodded columns and brick walls carrying the cantilever slabs.

Failure No. 26 is a school building at Roxbury, Mass., described in *Engineering News,* August 10, 1916. The concrete had set seven weeks and more, and while the collapse occurred in March, the concrete had been kept warm by the use of salamanders. This was a flat slab structure, with square rodded columns, and brick wall supports. There were three successive collapses; a large portion of the structure came down. This is similar to Failure No. 25, and the design had the same faults. I know that Mr. Sanford E. Thompson states, in the reference cited, that the cause of the accident was poor brick work; and his photographs show the corner brick piers criticised as the only thing left standing above the top floor level. He does not attempt to explain why interior columns failed because exterior brick walls were poorly laid up, nor why a calculated slab load of about 30 to 50 lbs. per sq. in. on the wall caused it to crush when his tests showed incipient failure to require three to five times this, and crushing to require more than ten times this. Mr. Thompson criticises the use of a brick wall to support a flat slab. There is nothing inherently wrong with resting a slab on a brick wall, so long as the slab is not designed, as a cantilever, so that the fault really lies in the habit of flat slab designers to ignore cantilever loads in exterior supports of flat slabs. It is very common to use rodded columns for exterior supports, the worst kind of column that could be used for cantilever loads; it is bad enough for interior columns.

Failure No. 27 is in the Braunstein-Blatt building at Atlantic City, N. J. This has not been published, so far as I know. A photograph was shown me of the failure. This photograph indicates that a round rodded column about two feet in diameter, supporting a flat slab floor, crushed and dropped down, allowing the floor to sag about two feet. The failure took place last summer or fall.

Failure No. 28 is a pier at Galveston, Texas, which collapsed in a fire, as described in *Engineering News Record,* November 18, 1920. This was a round rodded column flat slab building. The destruction in the fire was great, requiring reconstruction instead of replastering, as it would probably have required with properly hooped columns.

Failure No. 29 is another fire in a pier, similar to the last named, at the same place, and described in the same reference.

It is worthy of note that in all this array of failures every structure is of a type of design that I have been publicly condemning for fourteen years. I venture the assertion that no structure ever suffered general collapse (excluding fires in combustible materials of construction and bridge collapses where failure of one member of necessity drops the whole structure) where the type of design was a proper one. This in spite of the prevalence and

presence in many structures of high unit stresses, poor materials, poor workmanship, and local faults in design.

My critics demand that I give facts demonstrating the weakness and unreliability of rodded columns and dependence upon stirrups in beams. They claim that I gave nothing but theory and my private views.

I ask you, in all seriousness, does this tragic array of facts, that means the loss of dozens of lives and millions of dollars worth of property, mean anything to you? In the face of these facts, will you dare to issue another Joint Committee Report of the nature of the last and similar to the American Concrete Institute Standards, and thus perpetuate the outrageous standards of design that alone are responsible for all of this harvest of death and destruction?

You, and the rest of the men who are responsible for the creation and perpetuation of these standards, can continue to disdain to answer my arguments or take notice of them, but there will be a time when you will be compelled to make an answer—but not to me.

Yours truly,

EDWARD GODFREY.

SOME FAILURES NOT INCLUDED IN LIST

To the foregoing list there could have been added two filter roof failures, one at Baltimore, and the other at Cleveland. The Baltimore failure is described in *Engineering News*, Nov. 27, 1913, and in *Engineering Record*, Oct. 25, 1913. Neither account states what sort of columns were used. The photograph indicates that the usual type of rodded column was employed. The Cleveland failure is described in *Engineering News*, July 20 (p. 141) and July 27 (p. 186), 1916. This roof was supported on rodded columns, and the account states that the failure was in the columns. The unit stress was 250 lbs. per sq. in.

Another failure is the Prest-O-Lite Building, at Indianapolis, Ind. (See *Engineering Record*, Dec. 16, 1911 and *Engineering News*, Dec. 14, 1911.) This was a rodded column, flat slab building. The exterior columns had no flared heads; those on the sides had small triangular knees, but the corner columns had no spread of any kind. There were wide cantilever bands of rods over the tops of all of these exterior columns. The exterior girders were "pasted" on the top of the floor slab.

SOME RECENT FAILURES

Since the foregoing letter and list of failures was published two notable failures have taken place. One is the Masonic Temple at Salina, Kansas, the other is a large hotel building at Benton Harbor, Michigan. These failures

confirm most emphatically my condemnation of the rodded column, for both were rodded column structures. I was unable to learn the character of the beam reinforcement as regards provision for end shear, particularly in the case of the Salina building. Persons who knew this refused to divulge it either to myself or the engineering public. In the case of both of these buildings I was refused the privilege of making public comment and warning builders against their repetition.

Mr. T. L. Condron examined both of these buildings and made long reports on their failures. In neither of these reports is there any word linking up the rodded column in any manner with the cause or the extent of the failure. I quote from *Engineering Record,* Feb. 14, 1914, p. 217: "Mr. Condron agreed with Mr. McCullough that there is no justification for the use of concrete columns reinforced simply with four vertical rods and a few separated hoops or bands. Certainly such columns are dangerous to use, and it is surprising that so many of them have been used with so few disastrous results. The fact is that columns in buildings seldom receive the full load they are presumably designed for, but there is no part of a building that should be more conservatively designed than the supporting columns."

In 1915 at the American Concrete Institute Convention Mr. Condron expressed similar ideas regarding rodded columns, the type of column used in both the Salina and the Benton Harbor wrecks. His two reports are a virtual repudiation of the sound engineering principles expressed on the two other occasions. This would seem to leave the author of this present book quite alone in condemnation of the rodded column as advocated by all standard works and given in all standard specifications. This is perhaps true so far as men who write are concerned. There are many designers who eschew the rodded column.

The reason given for the failure of the Salina, Kansas, Masonic Temple (See *Engineers News-Record,* July 28, and Sept. 15, 1921, and *Journal, Western Society of Engineers,* April, 1922) is the sinking of forms in the mud long after the supported concrete had hardened. A large part of the structure, in a location not over these forms nor near them, collapsed in ruins. Part of this was on the other side of a row of columns that did not collapse. This failure took place 40 days after the last concrete was poured.

In the *Journal, Western Society of Engineers,* April, 1922, p. 112, Mr. Condron says: "The structure was carried entirely on falsework or shoring, up to the time of the collapse, and therefore the failure may be considered as one of falsework." On the opposite page a photograph shows all shores that would be visible removed below the second floor.

The Benton Harbor 200-room hotel took 30 hours to come down in ruins. There were a few columns in this building, interior columns, that were reinforced with spirals for part of their height. This is doubtless what retarded

the collapse of the building. But a few columns of a correct type of design could not be expected to stand up against the racking effect of a crumbling building. It was the outside parts of the building, where the rodded columns were located, that gave way first.

The presence of shoring or falsework, even though it may be capable of sustaining the direct weight of the concrete, has little effect in mitigating the extent of a failure where the supporting props (rodded columns) are practically useless in resisting swaying forces. Skew piers or abutments in arch spans have the same weakness, and failures once started in structures of these types generally result in complete collapse.

JAPANESE EARTHQUAKE

The Japanese earthquake demonstrated that some types of buildings will stand up even through a severe earthquake. Unfortunately, however, published accounts tell little of the type of building, particularly in reinforced concrete, that withstood the shock. It would be exceedingly illuminating if some observer had taken note of the type of column in the structures that collapsed and observed whether rodded columns or hooped columns predominated.

An article by T. Okubo, in *Concrete*, December, 1923, says of the reinforced concrete buildings, "Another group had the weak point in the column." He does not intimate the type of column referred to, whether hooped or rodded. He says, "Nearly all failures started in columns." He also refers to a photograph of a type of building that "suffered severely." Judging from the photograph it is the common type of building with rodded square columns and large window spaces between the columns. Mr. Okubo states that "Practically no building collapsed due to weakness in the floor system," and that "Steel frame structures protected by reinforced concrete wall, where well executed, came through with practically no damage." These facts are enough to prove that with tough columns, equivalent to steel columns, in the buildings in the earthquake zone, the history of the earthquake would have been quite different, much less disaster would have been recorded, where reinforced concrete structures were concerned.

SHORT LIVED "PERMANENT" BRIDGES

The following is quoted from an editorial in *Engineering and Contracting,* March 24, 1915: "To justify the use of designs prepared by it the State Highway Commission of Iowa has recently made a detailed inspection, analysis and field study of concrete bridges and culverts in service in that state, the investigation covering 82 bridges and culverts. Of the 82 lighter sectioned bridges examined—all built under county control—60 have clearly developed defects, 16 are sufficiently defective to be unsafe for traffic, par-

ticularly during periods of continued high water, and 2 have collapsed completely." This is a very poor showing for a type of structure that is held out to be permanent. It is true that steel highway bridges built about 30 years ago did not have a much better record than this. But those bridge builders did not have the same warning as to increased weight of rolling loads as recent builders have had, also they did not have experience from which to draw lessons. There is scarcely any doubt that type of design is responsible in a large measure for the poor showing of these reinforced concrete bridges. Standard design does not call for shear reinforcement that crosses the plane of possible cleavage in a reinforced concrete girder, hence shear cracks and shear failures are the most natural thing to look for.

Any accepted standards for steel bridges have always provided a member where there was a known stress and end connections for these members. The standard stirruped beam or girder does not meet this criterion. The rodded column corresponds to the cast iron struts of early metal bridges and belongs in oblivion with those cast iron struts.

CHAPTER VI

FAILURES OF CONCRETE

CONCRETE IN SEA WATER

Concrete exposed to sea water is very often subject to disintegration. Much has been said of the chemical effect of sea water on Portland cement. However there are many things that point to the conclusion that it is rather mechanical action that disintegrates the concrete. The formation of salt crystals in the pores of the concrete, as the water evaporates, has probably more to do with the breaking up of the concrete than the chemical action. The disintegration is usually confined between low and high water and is a surface action. If it were a chemical action, it would act below the surface of the water as well as above. Dry mealy (and consequently porous) concrete is affected to a greater extent than wet concrete.

The best precaution against the disintegration of concrete by sea water is to use good wet rich concrete and if possible to plaster the surface and trowel it smooth. All of this is to avoid pores into which water may enter.

ELECTROLYSIS IN REINFORCED CONCRETE

Destruction of reinforced concrete by electrolysis has been feared by many, and in fact there is no doubt that precautions need to be taken to guard against the destructive effects of stray currents of electricity. It is possible, however, that investigations and reports that have already been made have underestimated the effects of other conditions besides the electric currents. Dampness, salt, and mealy concrete, especially when they are all combined, will aid very materially in the disintegration of a reinforced concrete structure without the presence of electric currents. When these are aided by stray currents, there is scarcely any doubt that damage will result. These can be avoided, and when they are avoided there is probably little danger of destruction by electrolysis.

In order to avoid dampness in concrete, it should of course be made waterproof. The best way to make it waterproof is to make it of a 1:2:4 mixture of Portland cement, good clean sand and good hard gravel and to mix it thoroughly and wet enough to pour. Besides making a

dense and waterproof concrete this thoroughly covers and protects the steel.

Salt is sometimes used in concrete to prevent freezing in cold weather. Specifications for reinforced concrete should strictly forbid this. Salt is known to have a corrosive action on steel. Steel embedded in concrete made with salt has been found to be badly rusted.

In order to prevent electric currents from the wiring of a building from passing through the reinforcing steel the wires should of course be insulated. In one case where electrolysis was exhibited the electric wires were not carefully insulated, there being electric contact with the reinforcing steel through the medium of water of condensation and the conduits.

Dry concrete is a good insulator, the resistance to the passage of current being very high. Wet concrete is a fairly good conductor. When concrete is wet with salt water the conductivity is very much greater than when fresh water is used. This is another reason why salt should not be used in making the concrete.

It appears that direct current and not alternating current is to be feared in the matter of its electrolytic action, for the corrosion of the steel has been found to take place only where the steel is the positive electrode.

Some valuable tests and the account of the survey of a building damaged by electrolysis will be found in *Engineering News,* June 8, 1911.

EMBEDDED STEEL PIPES

Steel pipes embedded in concrete, left open for bolts, and gas-pipe posts for handrailing, have been found to be a source of injury to the concrete. These expand and crack the concrete, because they very quickly attain the temperature of the air due to the large surface exposed to the air and the small amount of metal. A thin tube will expand as much as a solid bar of the same diameter for a given rise in temperature. The low conductivity of concrete and the large mass make it impossible for the concrete to adjust itself to changes of temperature as quickly as the steel pipe, hence the differential expansion cracks the concrete, especially if the pipe is located near a corner.

Another case where differential expansion cracks concrete is where a pyramid of concrete is used around the base of a steel column to "protect" it. Here the exposed portion of the steel column very quickly attains the temperature of the air and conducts the same to the portion surrounded by the concrete.

FAULTY METHOD OF LAYING SIDEWALKS

One of the most abundant uses of plain concrete is in the laying of sidewalks. But the standard method of laying concrete sidewalks is faulty in the extreme.

In the days when flagstone pavements were common it was found expedient to have a bed of sand or ashes which could be leveled off to a flat surface before the stone was laid down. Concrete will fit any irregularities in the ground, and this bedding is unnecessary. Nevertheless this difficulty has been projected into the concrete paving business, and many are required to dig up six or eight inches of good solid ground, or even solid rock or shale, in order that the space may be refilled with loose stones or ashes. This is called drainage, and sometimes there is not the slightest amount of drainage to it, as it is the lowest surface in its locality. It would be better to leave the compact earth or rock undisturbed, only cutting away enough to get the necessary thickness of slab, then, with the proper kind of concrete slab, water would be practically excluded from the soil beneath.

The standard concrete for the base in sidewalk construction is a dry mixture which must be heavily tamped. This is another fault. A more dense concrete is made by a wet mixture that need not be tamped but can be leveled off with the trowel or shovel. Also a better bond with the top coat will result with a wet mixture, if the top coat is placed at the proper time.

The standard procedure in the matter of placing the top coat is to wait for some time after the base is placed and tamped, sometimes a wait of a day or of several days intervenes. This is a mistake. The top coat should be placed immediately after the base has been laid. The two batches of soft concrete coming together will then unite and bond perfectly. The top coat, instead of being the stiff mortar usually employed should be mixed wet, so that it also will be dense.

In the matter of troweling another error is made in the excessive rubbing to produce a glossy surface. This smooth surface will remain in this condition but a short time, for when it is weathered it soon becomes pitted. The reason for this is that the suction produced by the rubbing draws the cement to the surface, and neat cement will not stand much wear. Also the rubbing loosens particles of sand and stone which are partially set in the cement, and these subsequently are washed out.

The best treatment is to flatten the surface with the straight-edge and then to use the trowel only enough to give a smooth surface, which can then be roughened by drawing the trowel up vertically, producing

a choppy sea effect. Or a wooden float may be employed instead of the steel trowel.

In the matter of joints in concrete pavements another error is very commonly made. The usual method is to make sand-filled slits in the concrete base and then when the top coat is laid to make cuts in the same which are intended to be over the slits in the base. These cuts in the top coat are very often no more than mere impressions in the surface, and sometimes they do not come directly over the cuts in the base. A very much better method is to lay the pavement in alternate blocks.

The evidences of the faults in the common method of laying concrete pavements are these:

(1) Dishing of pavements by sinking in at the middle of slabs. This is due to uncompacted and porous bedding.

(2) Heaving of the pavement or the rising of the same from the surrounding ground surface. This is probably due to the abundant opportunity for water to collect and ice to form in large quantities in the interstices of the broken stone and ashes.

(3) Breaking away of the top coat from the base. This is due to lack of bond. It is very difficult to make a good bond to concrete that has already set.

(4) Pitting of the surface. The reason for this has already been explained.

IMPROPER MANIPULATION OF CONCRETE

While the great failures are almost without exception due to faults in design of structures or the plan of their construction, some failures, generally local, and some unsightly work, are due to improper materials and improper manipulation in the construction. This is particularly true of concrete, and it proves the importance of good materials, good workmanship and manipulation appropriate to these materials. The latter is not the same as good workmanship, for workmen may do their work in the standard way, the way they have been instructed to do it, and this way may be at fault, as it frequently is.

STIFF CONCRETE IMPROPER FOR REINFORCED CONCRETE

The Joint Committee report of 1920 recommends that a slump test of two inches be employed for heavy reinforced concrete work. This concrete would be very stiff. It would not flow. It could not possibly flow around reinforcing rods and cover and protect them. It is therefore totally unfit for any kind of reinforced concrete work. It is impossible to make concrete so stiff as this fill out the forms properly. The work

will be unsightly, and the steel will not be protected nor properly gripped. Concrete so stiff as this would be porous, and this, in many situations, would detract from the life and safety of a structure.

Stiff concrete has one feature in which it is superior to freely flowing concrete: this is its greater compressive strength. The only place where stiff concrete is safe to use is in massive work for foundations and the like, but in such work the high compressive strength is not needed. High compressive strength would be a desirable property in reinforced concrete construction, but density and the gripping and protecting of the steel are of equal if not greater importance than high compressive strength and are absolutely necessary in any reinforced concrete work.

Stiff, mealy concrete is particularly bad where the embedded steel is of thin section. The lack of covering and protecting properties allows the corrosion of the steel, and thin sections waste away relatively in much greater proportion than heavy sections. This is why metal lath so often fails, not only in exterior work but in interior work. The only kind of mortar that can be plastered is a comparatively dry mixture, and such mortar is not dense enough to protect the steel. The strong urge that is being exerted to revert back to mealy concrete, for the mere purpose of raising the standard compressive unit stress, is fraught with grave danger to the structures of the future.

TESTS NOT MADE OF MOST IMPORTANT PROPERTIES OF CONCRETE

Many thousands of tests have been made in an effort to discover the absolute maximum of strength, but practically no tests have been made on the concrete thus found to be of maximum strength to determine whether or not that concrete is dense enough and fluid enough to protect and grip the steel.

Unit stresses are being pushed higher and higher on the basis of these one-sided tests, and the contractor's difficulties are being multiplied by trying to force him to use an improper kind of concrete, a concrete much more difficult to handle and quite difficult to keep up to the standard of high strength required. And when a great wreck occurs in a brittle, rodded-column, stirruped-beam installation, the whole collapse of a building is sometimes blamed on a single batch of bad concrete. It is contrary to all reason that this should be the case.

Laboratory investigations that ignore the actual conditions of practice are of no value. If to produce the laboratory results conditions are imposed on the contractor which he cannot regularly fulfil, it is a waste of time to make and publish those laboratory tests.

A bucket of water more or less in a batch of concrete is too slender a thread on which to hang the safety of a structure.

A small fraction of the energy and funds expended on ideal laboratory tests, if directed toward the discovering of what a builder may expect with standard design, would long ago have forced revision of standard design to put it on a safe basis. When a standard specification (Joint Committee Report of 1920) recommends such absurd and unpractical proportions as 1:1.4:4.1 for concrete and sanctions the rodded column and the stirruped beam, it is time some horse sense be injected into the thing for the safety of lives and property and the conservation of the contractor's resources.

PROPERLY AND IMPROPERLY DESIGNED STRUCTURES—A CONTRAST

Two examples illustrate very forcibly what a single bad batch of concrete will do in a proper design and in one that is not proper:

In *Engineering News*, Aug. 29, 1912, p. 408, there is a description of the partial failure of a hooped column due to bad concrete. The column was giving way because of the weakness of the concrete in a part of the shaft of the column, but the structure did not collapse. An extra coil of steel was placed around the weak part of the column, and concrete was poured around this, and the building was saved.

In *Engineering News Record,* May 12, 1921, p. 825, there is a description of what happened because there was "a batch of dirty gravel" in a column. A seven story reinforced concrete building failed; two interior columns gave way. They were poured in July and August and failed the next January. The building was not loaded.

As a contrast with the first of the foregoing examples, Failure No. 27 in my list of 29, given in Chapter V, shows what will happen in a round rodded column. The floor dropped two feet. The assertion is here ventured that in repairing this damage the engineers used hooping. It is significant that no engineer ever repairs a failing rodded column by adding slender upright rods and square ties spaced a foot or so apart and thus making a standard and perfectly good column of it, if all standard works and specifications are to be relied on. Invariably the column is reinforced with a coil or hooping in repairing it.

An event of great significance occurred March 21, 1924, and is described in the *Engineering News Record,* June 5, 1924, p. 987. An attempt was made to wreck a reinforced concrete building being constructed for the Cincinnati Terminal Warehouse Co. by exploding a charge of dynamite at the base of one of the columns. This charge broke off the concrete outside of the steel coils of the column, tore a large hole in the

floor, and blew off the cap of the column below. But the integrity of the structure was not affected. "The concrete within the column spiral was uninjured and the green concrete above showed no injury except a hair crack in the sixth floor slab, which had been poured a day before the explosion. The injured concrete has been removed and it is expected that the work can be restored so as to be as strong as before." This was a hooped column structure. Compare this recital with a dozen or more reports where a block of wood, a handful of sawdust, a single batch of poor or dirty concrete, the sinking of shores into the soil, after months for concrete to harden, and many like alibis are given for acres upon acres of floor area and millions of cubic feet of buildings to collapse into utter ruin—the history of the rodded column and the stirrup.

HEATING MATERIALS FOR CONCRETE

Heating materials used in the making of concrete is fraught with danger from two different causes. One of these causes is that the heat of the stone and the consequent drying out causes it to absorb from the cement the water absolutely necessary for the hardening of that cement. The other cause is excessive shrinkage. Hot or warm concrete will shrink excessively and cracks will open up on account of this. I was called in on a building which I had designed. The builder pointed out some horizontal cracks that could not happen from beam stress. I asked him if he had heated the materials: "Yes, everything was carefully heated."

DEFLECTION OF STEEL WORK NOT ANTICIPATED

An example of improper manipulation of concrete in building construction that bears a significant lesson is the following. A building, the structural parts of which I designed, had long steel balcony cantilevers which supported reinforced concrete steps carrying the seats of a balcony. After it was built I was called in to look at the large cracks in the risers of these steps. I asked if forms had been left under the steel cantilevers during the pouring of the concrete of the steps and was told that this had been taken care of as a precaution. The cause of the trouble was the supporting of these cantilevers while the concrete set. If the concrete forms had been hung on the cantilevers, the cantilevers would have deflected while the concrete was wet. As it was the deflection of the steel cantilevers all occurred after the concrete was set, and the risers being on a large curve could not adjust themselves to the deflected position of the cantilevers, hence the cracks in these risers.

THE CONCRETE SHIP

There are some situations where concrete is not appropriate because of its inherent properties. One of these is the concrete ship. Of course ships can be made of concrete, and if there were no other material available, reinforced concrete could be used for serviceable vessels. But it is impossible to impart toughness to the hull of a concrete vessel, the property that will enable it to stand the punishment incident to its legitimate use.

The *Engineering News-Record* of Nov. 29, 1923, told of the retiring of a concrete ship by making a club house of it. A letter which I wrote on that occasion was refused publication. The contents of that letter emphasizes some lessons that may be drawn from laboratories and failures. It will be given here.

The reason why the concrete ship is a failure is exactly the same as the reason why certain standard types of reinforcement for beams and columns are a failure and have resulted in acres of floor space and millions of cubic feet of volume of wreckage of reinforced concrete structures. This reason is lack of toughness.

One would think that the meaning of tough and brittle would be obvious, and that Noah Webster and the Standard Dictionary would not have to be paged to make it clear. But it seems that on every occasion when I object to the brittleness of rodded columns and stirruped beams (their lack of toughness) someone feigns ignorance of the meaning and flies to the dictionary in a frantic effort to learn what these words mean.

In the closure to a discussion in Trans. Am. Soc. C. E., Vol. LXXXVI, p. 1223, the author of a paper makes the following statement:

"The Standard Dictionary defines brittle as: 'Liable to break; fragile; frangible; fragile is synonymous with having little strength, or endurance, frail, easily broken.' "

"There can be no question that in the general sense concrete is not brittle. The use of 'brittle', and its antonym 'tough', as given by Mr. Godfrey, terms for which there are no means of measurement, can lead nowhere."

Then the author of the closure goes on, in the same page, to state that work absorbed in producing ultimate stress is a measure of the toughness, and all that is necessary to make the toughness identical in rodded and spiral columns is to make the rodded column about three times as long as the spiral column. It might be safe in a laboratory to do this, if the testing machine were well guarded and the operator remains behind one of the uprights.

I never met a layman who did not understand immediately the difference between tough and brittle. A perfectly made rodded column or stirruped beam, in a laboratory, may be hard to break. But in a building, there are swaying forces in the columns, deflection in the beams, heavy and unavoid-

able shrinkage tension in the attached beams, unavoidable eccentricity of loading in columns and inequalities of the concrete of the columns. All of these are lacking in the laboratory test. They show up the brittle character of the structure that is not reinforced to give it toughness by hooping the columns and anchoring main reinforcing steel into the supports.

This paper, which takes up 130 pages of Trans. Am. Soc. C. E., thirty pages of which are occupied with the author's closure, adds a large amount to the literature which exalts the laboratory and laboratory results. The author says, page 1209, "The strength of laboratory test columns must be the basis of the design strength in any rational plan." The concrete ship was a laboratory plaything, demonstrated by laboratory tests to solve the marine problem. Of course the laboratory could only try to bite the hand off the china doll and it was very "tough," but when the said china doll fell on the rocks, there was a different story to tell.

The thirty and more reinforced concrete wrecks of history are simply the dropping of the doll of brittle but standard type of design of structures, and until the value of the laboratory test is correctly appraised, as merely incidental to the actual need of toughness in a structure, the wrecks will go merrily on.

IMPROPER CONCRETE REINFORCEMENT

Not only may concrete be inappropriately handled and used, and not only may a structural member lack in toughness by reason of improper design and placing of reinforcement, but the reinforcement itself may be weak and inappropriate.

Chain, cable, pipe, and hard steel that will not stand bending are, in general, improper reinforcement. Chain and cable stretch out excessively, and under loads which they would safely take themselves the concrete would be cracked by reason of the stretching out of this kind of reinforcement. Pipe and hard steel, because they may not be safely bent, are unsafe for many purposes to which reinforcement must be put.

Clips for splicing one rod to another, when both are embedded in concrete, are not proper, even though the splice may be capable of taking the strength of the rods. There will be a measurable set in the splice before the clips can come fully into play, and this would crack the concrete.

CHAPTER VII

THE FLAT SLAB

The flat slab is a type of reinforced concrete construction sometimes called girderless floor construction. In its simplest form it consists of a slab of uniform thickness supported on columns or posts spaced equally in both rectangular directions, the columns having flared-out heads. The reinforcing rods pass across the column heads in both rectangular directions and in both diagonal directions. Over the column heads the reinforcing rods are kept near the upper surface of the slab and midway between columns the rods are kept down near the under surface of the slab. There are variations of this general type of flat slab or girderless floor construction. Some have drop panels over the column head: some have deeper bands from column to column in the rectangular direction with a raised panel in the ceiling.

IMPROPER BENDING OF RODS FOR SHEAR

A constructional blunder is frequently made in the placing and shaping of the rods. This is to bend the rods from the top to the bottom of the slab with a sharp inclination. This is supposed to be to take the shear; but shear does not confine itself to any specified line or section either in a slab or beam. Shear exists wherever a load is being carried from a beam or slab to the support, and any system of reinforcement that fails to take account of this fact is erroneous in its conception. This error is one piece with the error of the stirrup, the short shear member, and the sharply bent up portion of the main rod in a beam; the only standard method of reinforcing for shear.

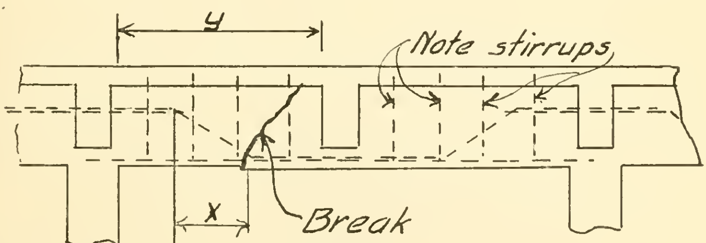


Fig. 1.
Typical Failure - Edison Building

Fig. 1 illustrates the error of confining the bend of main reinforcing rods in the distance x when the shear is constant for the distance y (because of the concentrated load). In a uniformly loaded beam the shear would be greater to the left of x, where no provision is made for shear. This figure shows the type of reinforcement used in the Edison Building. It also shows, by the wide open crack, the typical failure of the girders of that building. (See *Journal, American Concrete Institute,* August, 1915, pp. 662, 665, 666, and *Engineering Record,* April 17, 1915, p. 506, and *Engineering News,* Dec. 17, 1914, p. 1234.) It would hardly need a failure to demonstrate the error of this standard inhibition against flat bends in reinforcing rods. But it is these clear lessons of failures that are studiously avoided and ignored by investigators with the result that designing is not improved, and even standard specifications are written with clauses that tend to degrade rather than improve design.

Loads are not discriminating enough to confine their worst effect where the designer places his reinforcement. Designers must learn to place their reinforcement where the loads produce the greatest stresses. The Joint Committee Report and the American Concrete Institute Regulations make this error obligatory in all designing by limiting the angle of inclination of bent up rods. It is a virtue to violate this requirement: it has no substantiation in fact or logic. In general the greater the violation the better the design.

The proper shape for reinforcing rods that are needed in the top of a slab or beam at supports and in the bottom at mid-span is a flat bend or long sweep. The idea that a rod loses its usefulness for upper slab or beam tension the instant it begins to bend down toward the bottom of the slab or beam is another of the standard errors that have contributed to the long and shameful history of wrecks in reinforced concrete construction.

There is nothing inherently weak or unsafe about the flat slab type of design, but the standards of design in common use will not stand the test of analysis. Furthermore, the tests that have been made have been planned to show up the flat slab in the best light, and no critical tests have been made. This condition is the result of allowing commercial interests to dictate what is and what is not safe and the attitude on the part of engineers and authors to take their material and receive their education from the commercial catalog. When an engineer whose interests are commercial objects to the presence of the editor of an engineering paper, because he is an outsider, in a committee preparing standards of design, and then has two outsiders, advisers, beside him in the committee meeting, an occurrence which I observed, it is time that the education handed to us by the commercial interests be scrutinized.

THREE WEAK POINTS IN THE FLAT SLAB SYSTEM

The flat slab system, like Sampson, has its weak points, and, like Sampson, its sponsors have been wary about exposing these weak points. The weak points are three in number. They are (1) the exterior columns, (2) the exterior bays, and (3) a row of bays across a building. These will be taken up serially.

(1) The flat slab system is a cantilever system, that is, it derives its chief supporting value from cantilever action. In its simplest terms a cantilever load is an overhanging load. Now an overhanging load must either be balanced by other overhanging loads on the other side of a column or girder, or it must produce bending or twisting moments in the thing to which the cantilever connects. A large amount of bad detailing and designing is done because designers fail to grasp the importance of these elementary principles of stability.

In both steel work and reinforced concrete there are exhibited many examples of the failure on the part of designers to understand the principles of cantilever design. Self contained anchorage—anchorage into nothing—is very frequently employed in designs, a variation of the idea of lifting oneself by the boot straps. The Spang-Chalfant building which failed had cantilever slabs, very shallow in the middle of span, the whole cantilever load of the floor designed to be supported as a twisting load on the outside girders. This new and economic type of floor seems to have died with this example and my public exposure of the error of its design. (See *Engineering News*, Feb. 29 and March 14, 1912.) It did not get headway enough to take on authoritative approval, as the flat slab has.

In the case of the interior columns of the flat slab construction the cantilever loads are pretty well taken care of by reason of the balance of loads on opposite sides of the column heads and by the stiffness of the slab between columns (that is, the slab on the opposite side of the column from the loaded portion).

In the case of the exterior columns the balancing load is absent, and of course the slab does not continue to another row of columns and thus receive stiffness to resist cantilever stresses. The exterior columns are compelled to resist the bending stresses. In spite of this manifest and manifold weakness in the flat slab system, I have checked designs of floors, already accepted by a city building department, in which the exterior columns were far from being up to the requirements of that city's code for direct beam and slab loads with no bending moments considered. And, worst of all, the columns were rodded columns, the worst type of column employed in reinforced concrete work.

Some types of flat slab building have a small triangular bracket or knee at the top of the exterior column, a sort of rudimentary column head, and the reinforcing rods are spread away beyond this head. The edge of the slab has to support itself beyond this nominal column head. Sometimes, as in the case of the Prest-O-Lite Building which collapsed, there is a curtain wall or girder above the slab. These parted company in that wreck. It is astounding what is done in the way of designing with nothing more to support loads than the supposed magic in the name of a "type."

It is not unusual to see on the exterior wall of a building half column heads projecting out for future extension of floors. Doubtless the future floors would be designed as cantilever floors merely because the shape (when plastered onto these column heads) is similar to that of cantilever floors where rods pass over column heads.

RODDED COLUMNS PARTICULARLY BAD FOR FLAT SLAB CONSTRUCTION

Interior columns of flat slab construction are frequently reinforced with spiral or hooping. This is as it should be. Exterior columns are very frequently made of the rodded type, which is very bad. The rodded column is bad enough in beam and slab type construction, and it has proven its excessive weakness where loads are unbalanced. In the exterior columns of a flat slab structure the loads are all on one side of the columns, and the columns are subject to heavy bending stresses.

HEAVY BURDEN ON EXTERIOR COLUMNS PROVEN BY TEST

In the Bell St. Warehouse at Seattle, Wash. (See *Engineering Record,* May 13, 1916) under a test load the exterior columns cracked very badly, and the measured stresses were very high. The columns, of course, were not critically loaded. The test was a load on a floor, and columns would have to have all the floors loaded in order to receive their critical loads.

(2) The second weak point in flat slab construction is the exterior bays of the slab. The building referred to under (1), as having been examined and passed by the city, had the same reinforcement in the exterior bays as in the interior bays. There is a very much greater bending moment in the slabs of the exterior bays than in those of the interior bays. This is because there is not a balancing load outside of the line of the exterior columns to hold the slab approximately in the horizontal position.

Props at intervals along the edge of a slab do not constitute a proper support, even if these props have alleged column heads or small brackets taking hold of a pinch of the slab. The bands of rods are very much

wider than the column heads, and the rods that do not lie across the column head can do little toward supporting a slab supposed to be carried by that column.

EDGE OF SLAB CANNOT TAKE CANTILEVER STRESSES

Cantilever moments require full anchorage of the reinforcing rods at the point of maximum cantilever stress which, in this case, is the edge of the slab and the end of the rods. This is a condition practically impossible to attain in the common standard where the edge of the slab is at the outer line of the building.

Girders between outside columns offer proper support for the exterior bays of the slab, if the girders are under the slab or if they have stirrups at close intervals for the entire span. These stirrups are not

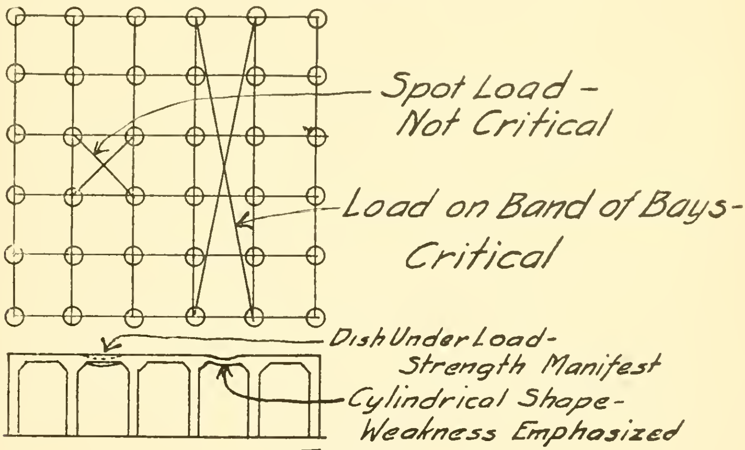


Fig. 2.

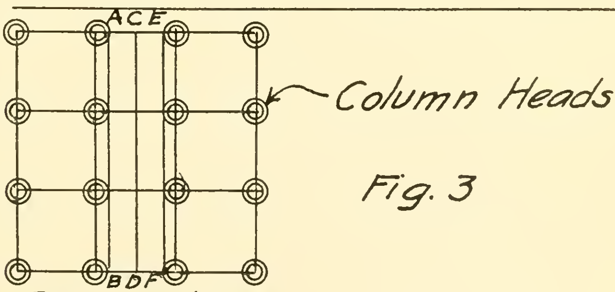


Fig. 3

Plan of Columns

for shear, but are to hang the slab to the girder in cases where the slab is near the bottom level of the girder. They should be looped below the reinforcing rods of the slab.

Those who have had the testing of flat slab structures have been care-

ful not to test a line of exterior bays. This would be a critical test, especially in a floor where there is no girder between the exterior columns.

CRITICAL TESTS AVOIDED

(3) The third weakness in a flat slab building would be exhibited if a test were ever made of a line of bays completely across a building from one side to the other in a direction at right angles to that of the lines of columns. That is the space to be tested would not be a line of bays similarly located to the black squares of a checker board but a strip of the floor bounded by two lines of columns and the outside walls of the building. Anyone who has followed my discussions on this subject will doubtless understand why it is necessary to go into detail in defining this exceedingly simple thing. Great efforts have been made to misunderstand me when I have previously defined this critical test of a flat slab floor.

When a complete line of bays across a building is tested as described in the foregoing paragraph, the tendency is for the floor to assume a cylindrical shape and not to be dished in spots. Loading of interior bays, with the surrounding bays idle, causes the slab to be dished in under the load, and the unit carrying capacity is very much greater than if the slab bent in a line parallel to the supporting lines of columns and assumed the shape of a portion of a cylinder.

Fig. 2 illustrates the standard type of spot loading and the critical test—never made.

A lot has been written on the supposed great strength of flat slabs. Some of the defenders roam all over a structure from roof to column foundation in the cellar to gather up elements of strength to support the bending moments they find across the head of a column. (See *Engineering News,* Dec. 24, 1914, p. 1275.) Bending moments that occur in one direction are resisted by sections at all different angles from that section, including the impossible angle of 90 degrees.

Tension on concrete, the thing that takes five or ten times as much actual stress as the tension in the steel of a flat slab, is waived aside as though it were non-existent, so that the magic of the "type" will loom large in the test results.

MISLEADING MATHEMATICS OF THE FLAT SLAB

A lot has been written on the mathematics of the flat slab, most of it mathematical yiddish that is utterly worthless. It is like carefully triangulating the distance around the world and subtracting this from the circumference in order to find the distance between two nearby points.

There could be no quarrel with a mathematician amusing himself in

working out intricate formulas, if his results were correct, or if there were no other way to find the answer. But the mathematical jugglers who have attacked the flat slab problem reach results that are not even approximately correct, and there is an exceedingly simple way of reaching the correct result. No mathematician has ever disputed the correctness of this result: its simplicity is manifest to all.

Without going into the demonstration of this formula that has never been disputed it will be stated. In Fig. 3 the sum of the numerical value of the bending moments along the line AB and along the line CD must be equal to the total weight on the rectangle ABFE multiplied by one-eighth of the distance AE.

No commercial method of designing flat slabs provides reinforcement across the sections AB and CD that will satisfy this undisputed criterion. Some do not even supply a respectable fraction of this reinforcement. The building codes follow these methods. Books approve them. The only standard that ever met this criterion was the Joint Committee Report of 1916, and this is one of the reasons why this "Final Report" was revised and repudiated a very few years later. Another reason was to eliminate my dissenting note on rodded columns and so-called shear members.

The apparent good showing of flat slab tests is due to two things. One is the unfair way in which the tests are planned, the other is the tensile strength of the concrete. The steel reinforcement is aided by the tensile stresses in the concrete, and because of the many directions in which stresses may travel more cracking would be necessary to expose the weakness than in the case of ordinary slabs and beams. So long as the slab is entire, test results show up very well. A notable case of failure in a large flat slab building proves what may happen after the slabs crack in the various weak sections. The floors in this building were sagged about three inches after cracks formed around the column heads and across the slabs in weak lines.

As a closing word in this chapter it is believed that a warning against the practice already referred to on page 132, of leaving half column heads projecting out of a wall, as corbels for the support of additional floors, is in place. This is bad construction and will probably have its harvest of failures. Deflection of the floor slab is almost sure to break off these corbels. Cantilever action is impossible. Corbels are of doubtful efficiency in beam and slab floors; in flat slab floors they invite disaster. New columns should be built.

CHAPTER VIII

STEEL WORK

Engineering work in steel is chiefly included under the following heads: trusses, girders, beams, columns, tanks, standpipes, stacks. Columns, tanks, and standpipes are considered in separate chapters. Trusses of course consist of compression members and tension members. The former would be included under columns. Tension members that fail are usually deficient in sectional area. The end details of both tension and compression members are often inadequate to take the stress of the member. Bracing of trusses, girders, and beams is of the utmost importance as an element in their strength. The need of bracing is treated in another chapter. Bracing and its connection to the part braced may be and often is insufficient. End details of truss members are also very frequently lacking of the necessary strength. These matters will not be taken up in this book. A number of the common errors in details were described and criticised in an article of mine in *Engineering News,* April 11, 1907. The substance of this article is reprinted in my book "Steel Designing."

SUDDEN BREAKS IN STEEL CRYSTALLINE

When structural steel breaks in service it almost invariably shows a crystalline fracture. In new steel this very frequently is taken to indicate that the steel is faulty, burnt or overworked. In old steel it is usually taken to indicate that the steel has crystallized from constant use. In general both of these notions are wrong. I have many times seen steel specimens cut out close to a crystalline fracture (caused by over-stress or the dropping of a structural member) and tested. They can usually be bent over and hammered down flat without sign of fracture, and in tensile test will show large stretch and reduction. Suddenly applied stress on any steel, or even on wrought iron, will cause it to break off sharp and with a crystalline fracture. It requires time and the gradual increasing of the load to cause steel to draw out as it does in a testing machine. Usually a break in service is a sudden one, the full load being applied at once. Even in failures that have required a long period of time and many repetitions of the load the conditions of the testing machine are not duplicated. Such failures very often show a portion of the fracture rusted and the remainder bright, all of it being crystalline. In such failures the first application of excessive load no doubt causes a minute crack. Successive applications extend this crack, until the piece

is weakened so that one application of the load completely severs it. This kind of failure usually occurs when a piece is alternately bent back and forth, as in a car axle, or as in the standpipe plates referred to in Chapter XI, or the suspension bridge hangers referred to in Chapter XII.

OLD SPAN FAILS UNDER LOAD FREQUENTLY CARRIED

On May 7, 1900, a plate girder span in Allegheny went down under the weight of a locomotive, after standing for 30 years or more. It broke apart in the bottom flange about one-third of the span from the end. Nothing indicated that the top flange had failed by buckling. The bottom flange consisted of two $3'' \text{x} 3'' \text{x} \frac{7}{16}''$ angles and two $24'' \text{x} \frac{1}{4}''$ plates. Two lessons can be learned from this failure: one is that long use and the fact that a structure carries its load and "gives satisfaction" are no indication whatever that it is safe; the other is that wide flanges with small flange angles are not good. This appears to be a common practice in Europe. Fortunately it is not practiced now in America.

VARYING STRENGTHS OF TOUGH AND BRITTLE SPECIMENS

The cable hitch shown in Fig. 1 failed under a load of about 6,500 lbs. The two straps are $3'' \text{x} \frac{5}{8}''$. Similar straps, when pulled in a testing machine, withstood ultimate loads all the way from 6,300 lbs. to 32,000 lbs. on a single strap. The bending moment at the corner of this strap is $\frac{1}{2}$ x multiplied by the pull on a strap. In this case at 10,000 lbs. per sq. in. extreme fiber stress the load per strap should not have exceeded 1,440 lbs. The load carried by the cable was then more than double what the hitch should have been called upon to carry. There are several instructive features about this failure. One is that tests in a machine on such a detail as this are of little value, because they show such seemingly erratic results. Another is that sharp bends may reach their ultimate strength at a fiber stress but little above the elastic limit of the steel. The test bar which failed at 6,300 lbs. had an extreme fiber stress of about 43,000 lbs. It cracked at corners *a* and *b* and did not straighten out. The bar which stood 32,000 lbs. of ultimate load partially straightened in the machine, and of course the bending stress of the original square-bent bar was not present at failure. A hasty conclusion might be that the steel was at fault, and in fact there must have been some difference between the steel of the several pieces tested. A more correct conclusion is that the detail is at fault and that a heavier bar should have been used; also there should not have been sharp bends at *a* and *b*. Some might say that the bar should have been made to fit the flange of the I-beam. It is precarious to rely upon such a fit. The bar might appear to fit and yet not be in contact.

The sharp inner angles at *a* and *b* are sources of weakness. Being bent over the sharp corner of an anvil they have the effect of a nick in the steel, and steel will bend or break in tension under much less load at a nick than in the plain bar. Further, the tendency, even in good tough steel is to break at or near the elastic limit of the steel, where it has been nicked with a hammer. While some specimens of steel would show toughness enough to straighten out partially under tension, others would be more adversely affected by the nick and would break sharp. The specimen which stood 32,000 lbs. of tension is no criterion whatever of the strength of this hitch. It is simply a specimen of steel of unusual toughness, or it perhaps was struck the final blow in forging when it was at a higher heat than the others. Safety should

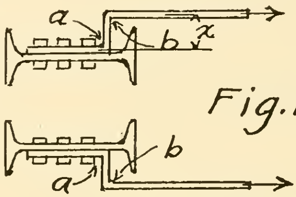


Fig. 1.

not depend in any degree upon such trivial circumstances, but the calculated unit stress on the strap should be such as to have a good factor of safety within the elastic limit.

One exceedingly important lesson of this failure is that theory may sometimes be vastly more valuable than tests. Another equally valuable lesson is that averages, where results are widely varying, are dangerous, when applied to design. In this case the average of six tests was about three times the minimum and about *thirteen times the calculated safe load*. A factor of safety of six, based on this average would have given a safe (?) load twice as great as it should be. A factor of safety of four would have given unit stresses close to the elastic limit. It is instructive here to compare standard analysis of so-called reinforced concrete columns (those with slender longitudinal rods in them). There, on the basis of absurd averages, factors of safety of three or four are employed and a wreck every few months demonstrates the absurdity of it; and yet men rake the ruins in a blind search for evidence of weakness in some trivial hair upon which the integrity (not the safety) of their structure depended.

IMPORTANCE OF OBSERVING PRINCIPLES OF STABILITY AND EQUILIBRIUM—QUEBEC BRIDGE

Steel work, and in fact any structural work, may be faulty in design because of violation of principles of stability and equilibrium. Some of the greatest wrecks in history have been due to neglect of these principles. The

first failure of the Quebec Bridge in 1907 was due to lack of bracing in the great traveler with which it was erected. The second failure, the dropping of the channel span, which occurred in 1916 was due to neglect of a principle of equilibrium just as simple and just as vital as the other. Description of this failure will be found in *Engineering News,* Sept. 14, 1916, and later issues.

The suspended span of this bridge was being lifted from the barges on which it was erected. The span was 640 ft. long and weighed 5,200 tons. It rested on four shoes which were each resting on pins: each shoe rested on two pins, the upper one being normal to the plane of the truss and the lower one being at right angles to this. A heavy steel casting in cruciform shape rested on the lower pin and supported the upper one. The commonly accepted explanation of the failure is that this steel casting broke under stresses figured to be about 20,000 lbs. per sq. in. These steel castings proved their strength before the failure by sustaining double load.

The supports of the shoes were in a condition of unstable equilibrium. The short girders supporting the shoes were swinging on pins whose centers were only 2¾″ above the center of the pin in the shoe supporting the suspended span. If the pin of that shoe were not located exactly in the vertical plane with the suspending pin, to the smallest fraction of an inch, the shoe and girder would take a slanting position. The longitudinal pin on which the shoe rested was polished and greased—ideal conditions for the sliding of the shoe given a very slight slant to the shoe. If the shoe were out of place about $\frac{3}{16}$ of an inch, the slant would be sufficient to cause it to slide off its support.

Fig. 2.

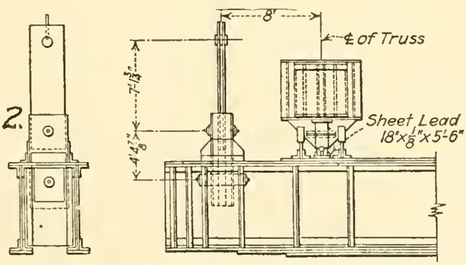


Fig. 2, was published in connection with a letter of mine in *Engineering and Contracting,* Oct. 25, 1916. The condition of this shoe on the rocking girder was similar to a pail that is hinged just above the center of its height or of one who sits on a swing and attempts to hold himself from tilting by grasping the rope near the seat.

The breaking of the cruciform steel casting between the pins is easily explained. After the girder had rocked through a small angle the shoulders of the upper and middle shoe would come together at A, Fig. 3. The pres-

sure on the small surface in contact would be enormous, as it would be a line contact. This great pressure could readily crush and break the casting. It is said that the shoe of the suspended span slipped off in a diagonal direction. This can readily be accounted for by assuming that the first contact between the shoulders of the shoes was at one corner. It is also said that stop plates at B (toward the suspended span) on the shoe which failed first were not broken off while those at D were broken off. If it is assumed that the first rotation of the girder was in the direction indicated by the arrow at E, and that the cruciform shoe was broken by crushing at A and rupture at

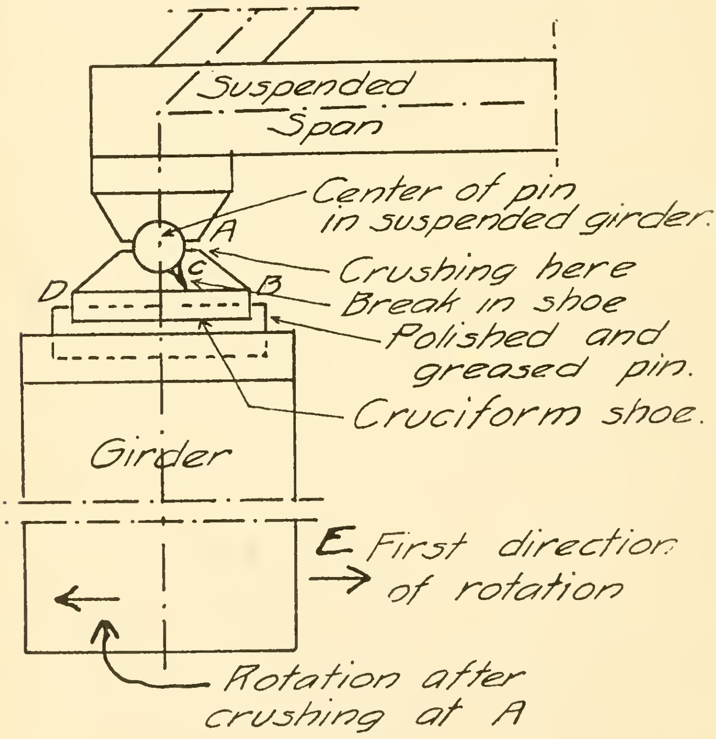


Fig. 3.

C, the load would immediately be transferred to the right side of the center of the girder, and this would reverse the rotation indicated at E, causing the bridge to slip off on the right hand side of the girder.

There is every reason to believe that this violation of the principles of equilibrium and stability was the cause of the wreck of this span rather than insufficient strength of a heavy steel casting which would, from known principles of the action of steel, have adjusted itself even to theoretical adverse conditions by its toughness and by deflection of the parts. There was enough

metal almost directly over the bottom pin to take an enormous load before failure would result, but rocking of the upper pin created a condition that would cause local crushing and rupture. This condition was not repeated in the rebuilding of the structure.

CURVED GIRDER—ORPHEUM THEATER FAILURE

Another type of instability in steel work was illustrated in the failure of the Orpheum Theater in Brooklyn, N. Y. This happened in 1913 and is described in *Engineering News*, Jan. 30, 1913. Reports as to its cause appeared about a year later, but none of these reports mentioned the great structural blunder in the design. This theater is shown in Fig. 4, a sketch published in *Engineering and Contracting*, Feb. 18, 1914, in connection with a letter of mine. A large facia girder having a span of 72 feet and an offset of 12 feet was supposed to carry the front of the balcony on which a concrete floor had been laid. This girder flopped down and bent the columns to which it was connected, two slender columns carrying the roof. This brought the

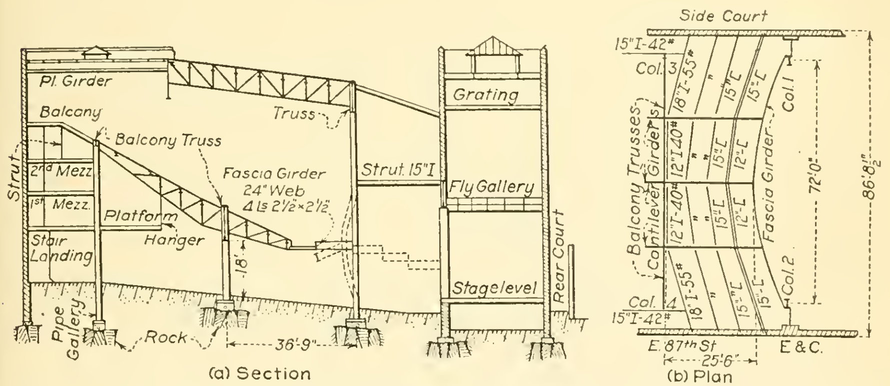


Fig. 4. Section and Part Plan of Orpheum Theater, New York, Showing Type of Construction and Manner of Failure.

roof down on the balcony and wrecked it, of course breaking down the balcony girders and breaking the steel with the customary sharp breaks.

Theories regarding this failure include alleged weakness of splice plates in the balcony girder that received the shock of the falling roof, because of the sharp break in these splice plates. Tests did not show these plates to be weak. They also include a theory of the rotation of the girder supporting the balcony cantilevers due to the deflection of the latter. If steel were so fragile and brittle as this a great proportion of steel structures would be in ruins.

Any beam or girder that is curved in plan must depend for its support either on cantilever stresses at the ends or on intermediate supports. This

girder was not provided with either resistance to cantilever stresses at the ends or intermediate supports: the girder was carrying the ends of the balcony beams and a concrete slab.

An article of mine that appeared in *Engineering and Contracting,* Dec. 8, 1915, contains reference to a large number of errors in details of steel work. This article is reprinted below for the lessons which it contains.

Reprint from *Engineering and Contracting,* Dec. 8, 1915, p. 438—.

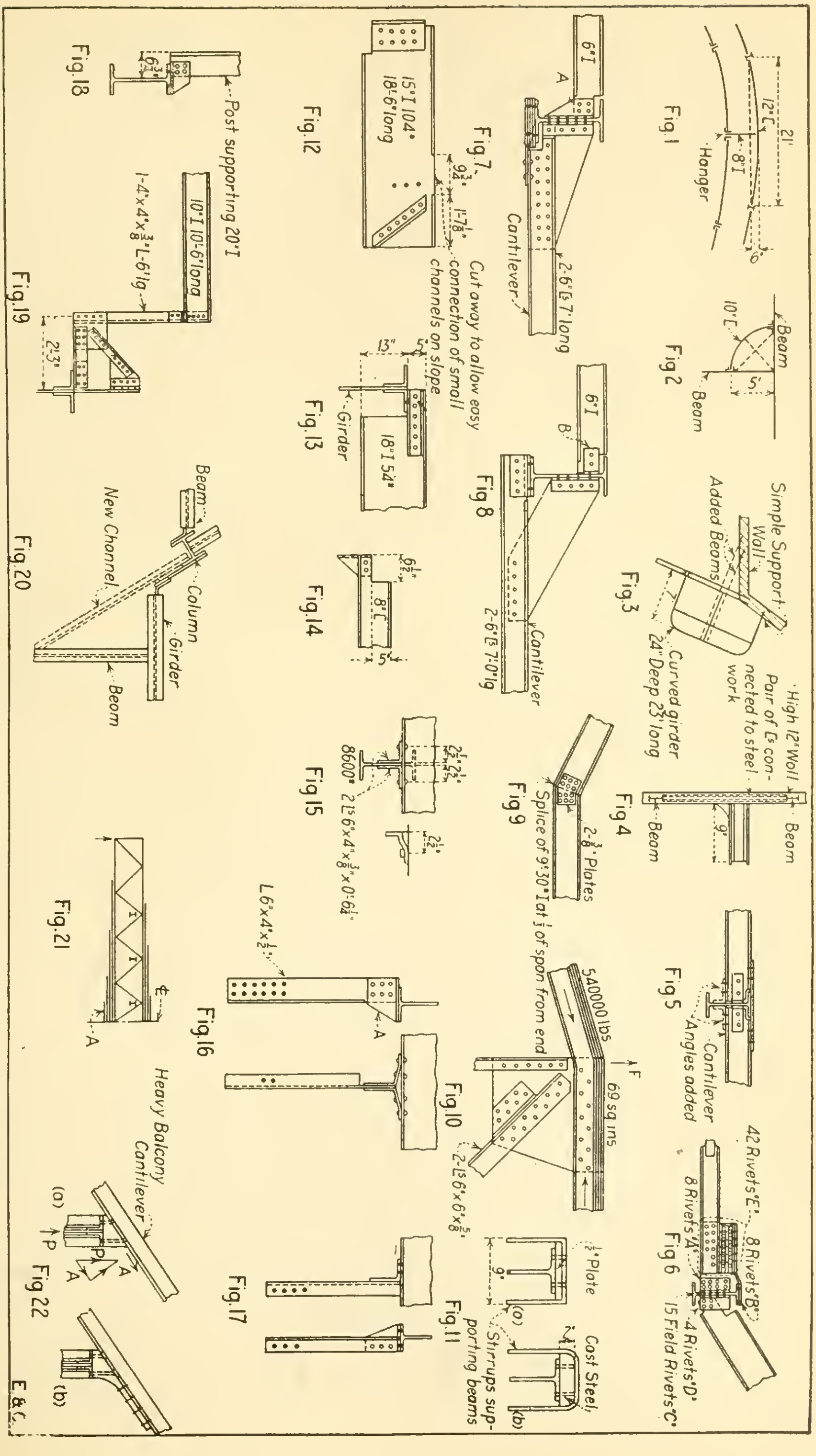
ERRORS IN STRUCTURAL STEEL DESIGNING

Books have been written in a high key. They descant learnedly on higher structures, ellipses of stress, secondary stresses, influence lines, and many like things. The writer has made a study of structural failures. He has never seen the slightest evidence indicating that any of them have been due to the lack of the things mentioned in the second sentence of this article. He has seen wrecks without number, real and potential, that indicated failure on the part of the designer to apply ordinary horse sense in their design. This article is a collection of errors in design. The errors all occurred in one set of drawings, but they are not for this reason an exhibition of one or a few designers' peculiarities; they are common—very common. No single designer is responsible; the responsibility lies higher and broader. Designers cannot all be independent investigators. Most of them take for granted what they are taught and what they read in standard books, or they consider as non-essentials the things that are omitted in teachings and books. The writer would like to see serious study given by structural engineers to the errors pointed out in this article.

Figure 1 shows a 12-in. channel, with a span of 21 ft., curved, with an offset of 6 inches. The channel had ordinary end connections. When the designer was asked what would prevent this beam pivoting on the end connections and flopping down, he replied that "it was held at the middle by the 8-in. I-beam acting as a cantilever." This "cantilever" was connected with three rivets to a light hanger. To emphasize the fact that this is not a cantilever would seem to the writer to be superfluous, if it were not that he had the greatest difficulty in demonstrating this and like points to the designer. As a cantilever it would have the greatest bending moment at the hanger and in the hanger. The end connection is quite inadequate to take bending and the hanger is unsuited.

To remedy the error the channel was made straight, thus allowing the curve to be made in the plastering.

Figure 2 shows another sample of a curved beam which is even worse than the first. This beam was to carry a portion of floor and a wall. The designer marked this 10-in. channel to have "stiff connections," as if stiff connections would relieve the wrenching effect on the beams to which the channel is connected. A curved beam is in reality a cantilever, but here is a cantilever



Figs. 1-22. Examples of Errors Found in Checking Structural Steel Designs.

branching out of the side of a single beam. To remedy this a straight beam was introduced between the ends of the 10-in. channel and a cantilever from the corner of the quadrant, as indicated by the dotted lines.

Figure 3 shows another curved girder which is worse than either of the first two. This girder is 24 ins. deep and 23 ft. long. It has a sort of gallows connection at one end (of which more will be said later), and it rests on a wall at the other end. It supports a large area of floor overhanging the supports a distance of 7 ft. The beams shown dotted were added at the writer's suggestion.

Figure 4 shows a greater overhang than that to which attention was called in the last paragraph, but the gallows detail is at both ends. The designer stoutly maintained that this detail was good because the channels in the wall would take the bending. But what about the high unbraced wall? A cantilever must have a back anchor and a gallows frame must be "planted" firmly at its base. This is not stable on either basis. The cantilever has no back anchor and the gallows was simply connected to a wall beam at its base.

The practice of "losing" or ignoring the cantilever stress just where it reaches a maximum is very common. It is exemplified in a large number of these cases.

The remedy for this case was not a simple one. It required alterations in the architectural features of the building which allowed the introduction of support for the cantilever, in front of the wall line.

Figure 5 shows another example of failure to take care of cantilever stresses. The top flange stress was provided for by means of a plate across the beam, but the bottom flange stress was totally ignored. To remedy this error the writer introduced angles (shown dotted) to take the bottom flange compression.

Figure 6 shows a cantilever which is connected to a cross beam with 73 rivets ("A," "B," "C," "E"). *The bending moment of the cantilever must all be carried by the four rivets, "D," which could be put in the space of the palm of one's hand.*

Figure 7 shows a cantilever which is connected to a cross beam by 39 rivets. The four rivets, "A," take the bending moment into the 6-in. I-beam.

In Fig. 8, 25 rivets are used to connect the cantilever to the cross-beam. The two rivets, "B," take the bending moment into the 6-in. I-beam.

The last three cases are examples where excellent provision is made for cantilever stress up to a certain point—the point of maximum bending moment—and then the matter is dropped as though the cantilever stress had no existence whatever. In the example shown in Fig. 6 the top plate over the cross-beam was extended to connect with the inclined beam. In the examples shown in Figs. 7 and 8 it was recommended that a pair of 6-in. chan-

nels be substituted for the 6-in. beam, with suitable connections to take the bending.

Figure 9 shows a splice in a heavy 9-in. I-beam at a break. The splice was inadequate. In cases of beams on broken lines, such as this, the designer should use double channels, separated so as to permit the insertion of a splice plate as deep as or deeper than the channels themselves. Flange splices are not good where the plates are bent, hence the need of deep and thick web plates, with plenty of rivets to take the bending. This splice, however, was reinforced with heavy bent plates, since the bend was downward.

There were many examples of flange splices in broken beams, such as that shown in Fig. 9. Some of the beams were larger than this; some of them had bent plates developing the strength of twelve rivets. How any designer can imagine that the strength of twelve rivets can pass around the sharp corner of an 8x½-in. plate is a mystery. The bend in these beams was upward, so that the knuckle action would force the plate away from the beam in both the compression and the tension flanges.

Figure 10 illustrates a particularly bad case of "bends." Here a stress of 540,000 lbs. is expected to pass around a sharp corner in a very heavy chord. It is seen that there are but two or three rivets near the point where the upward resultant, "F," acts. These are totally inadequate to take that resultant. Apart from the inadequacy of these rivets to take that resultant there is the further fault that at the bend the cover plates and the horizontal legs of the angles will tend to bend upward under stress and throw their load on the vertical legs of the angles.

It would have been very easy to extend this gusset plate to the left about as far as it now reaches to the right of the bend. With this trifling expenditure the strength of the girder would have been enormously increased although the detail would not be ideal because of the large area of metal in the bent plates.

Figure 11 (a) shows how two heavy beams were hung across a girder. Here is a sharp bend, with the brittleness which it is liable to cause in the forging and a wide thin plate acting as a cantilever. The detail was changed to that shown in Fig. (b).

Figure 12 is an astounding example of simplified detailing. This is a 15-in., 104-lb. I-beam, which is "some beam." The entire top flange was cut away merely to allow an easy connection for some small channels on a slope. This hardly needs comment. Beam flanges are useful not only near the middle of span but also near the ends to give stiffness. The top flange of a beam is especially useful near the ends of span.

Figure 13 shows how an 18-in. I-beam was cut away for convenience in detailing and its end connection made exceedingly weak. The supporting power of a girder is at its middle plane. The middle plane of this girder

touches only the tip end of the 5-in. tongue of this 18-in. beam. The rivets are remote from this tip. The outstanding flange of an angle should not be required to carry a load. Seventy per cent of the web of this I-beam is cut away, leaving but little to take the shear. A sharp corner sheared out of a web means a possible incipient crack in that corner. This detail could not be much worse. The I-beam could have been extended and could have had a good web connection to the side of the girder.

Figure 14 resembles the preceding example. To remedy this defect a bent plate was used with four rivets in a line in the web of the channel, in place of two.

Figure 15 shows how a beam was hung under another beam. Such details are sometimes hard to avoid. The fault is not so much in the kind of detail as in the weakness of the connecting angles. These angles tend to bend and should be of metal thick enough to resist that bending. The lever arm for the force causing this bending is one-half of the gage, or $1\frac{1}{4}$ ins. The bending moment is $4{,}300 \times 1\frac{1}{4} = 5{,}380$ in.-lbs. On a plate $6\frac{1}{4} \times \frac{3}{8}$ in. this gives an extreme fiber stress of about 37,000 lbs. per square inch. These angles ought to be of $\frac{5}{8}$-in. thickness.

Figure 16 shows a hanger for a heavy I-beam. It is faulty in many respects. It is eccentric. The metal of the angle is about half cut and punched away in the section at "A." The rivets are very poorly placed to take the load. Four of these rivets could not possibly act until the other four had failed. This is indicated by the drawing. In a tension detail such as this all of the rivets which are expected to act in supporting load should be near the heel of the angle, and the metal of the angle should be thick.

Figure 17 shows another hanger which supports a considerable load. This is eccentric in every way that it is possible to make it. With an apparent strength of four rivets in tension it has in reality the strength of only two, and the pull on these is one-sided.

Figure 18 is an example of a post resting on a beam. No attempt whatever was made to bring the center of gravity of these posts over the center of the supporting beam, as they should be. In this case a large part of the section of the post is entirely off the flange of the beam. Some of the posts were not even milled but were $\frac{1}{4}$-in. from the surface of the beam and were connected to the beam with a little lug and a small group of eccentrically placed rivets.

Figure 19 illustrates the worst example of an eccentrically placed post. About 40 rivets were used in this heavy triangular brace and the whole connection could be swayed with a crowbar.

Figure 20 shows a very bad case of eccentric load on a column. The girder in this case was at a considerable distance from the column line, a

plate being extended to meet it. The writer added the new channel which is shown dotted in the drawing, to take the bending moment.

In other cases where wall columns were eccentrically loaded their sections were reinforced with side angles and plates.

Figure 21 shows an error in the use of cover plates. The cover plate "A" reaches only half way across the panel of the girder. It is evident that the stress in this panel is constant for the entire panel. Reinforcement added for half of that distance is simply wasted.

Figure 22 (a) shows a heavy balcony cantilever, resting on a cross girder, the connection being made with a cast iron triangular block.

The reaction, *P*, of the cross girder is vertical. This has a large component, *A*, for which no provision was made. The crooked holes indicated would have to be gouged out to get a bolt through them, and bolts in such holes would be of little value. In any event four bolts would not be enough to take the component, *A*. The writer had cast steel blocks made, as indicated at Fig. 22 (b). These blocks could be riveted to the cantilever without danger of cracking the casting and the rivets would take all of the stress in a direction parallel to the flange of the cantilever.

THE GLEN LOCH FAILURE

An example of a disastrous wreck and failure of a structure in steel, after the structure had stood for many years in spite of its faulty design, is the Glen Loch Bridge near Philadelphia. This is described in *Engineering News,* Dec. 19, 1912. A box girder rested on a column, the two webs of the box girder straddling the column. But there were no stiffeners over the column section nor diaphragm between the girder webs to take the load of the column into these girder webs, a very serious omission in the design. After standing for twenty-two years failure occurred by the column shaft punching through the cap plate of the column. A fatal wreck of a passenger train was the result. Proper details in the design would have avoided this.

For twenty-two years this structure could have been held out as an argument by those who say that because a structure stands and carries its load it has proven itself to be safe. There are many reinforced concrete structures that furnish this argument.

STEEL DESIGN ON SOUND BASIS—UNSOUND RECOMMENDATIONS

Original design in steel work as defined by standard specifications is on a very much sounder basis than reinforced concrete. Structures built in accordance with these specifications, when the bracing and the details of design are properly taken care of, are permanent and safe for the loads described in those specifications.

When the loads carried by a given structure begin to exceed those for which it is designed, the question of the safety of the structure becomes important. It is pure fiction to say that steel structures are designed with a factor of safety of three or four. Many parts of structures do not have a factor of safety of 2.5, and the crippling point of many of them would be reached if the load on them were doubled.

In *Engineering and Contracting,* June 3, 1914, p. 645, the following letter of mine appears.

Comment on Mr. J. E. Greiner's Paper, "Rolling Loads on Bridges."

To The Editors:—A modern trend in the matter of railroad bridge designing is shown in a recent bulletin of the American Railway Engineering Association, namely, Vol. 15, No. 161. In a discussion on "Rolling Loads on Bridges," Mr. J. E. Greiner makes some recommendations which, if taken seriously, would totally demoralize railroad bridge design. Based on opinion solely, for no data nor tests whatever are quoted, Mr. Greiner makes the statement that railroad bridges can, with perfect safety, be overloaded all the way from 50 to 100 per cent. And other engineers agree with him, on the same basis. It is a good thing that there are rules and laws which require that stability be grounded on something better than the opinion of men: there ought to be more of them, especially the laws.

In Vol. 9 of the American Railway Engineering and Maintenance of Way Association, pp. 219 and 289, Mr. Greiner gives a report of his committee on "Iron and Steel Structures," so that these deliverances may be taken as the opinion of the Association. Mr. Greiner says, "Unit strains in tension to the extent of 26,000 lbs. in structural open hearth steel and 22,000 lbs. in wrought iron will not, in themselves, be sufficient justification for suspending traffic or condemning the structures." Again he says, "That means if you cut down speeds you can run heavier overloads. It would mean practically taking a 60 ft. girder span at full speed on an overload of about 71 per cent, while at slow speed the overload would be something like 122 per cent."

In the bulletin above referred to Mr. Greiner states that "we know" from numerous tests and long experience that properly designed bridges in good condition will safely withstand an overload of 50 per cent without any traffic or speed regulation, and if speed is regulated an occasional overload of 100 per cent. No tests are cited: the experience is not described. This is stated as more conservative than has been the successful practice of a number of railway engineers. Mr. Greiner further states, "An E-50 American Railway Engineering Association Specification bridge is a good and economical type with sufficient strength to safely carry, in regular unrestricted service, the heaviest locomotives that can be safely operated without a possible complete revision of present standard clearances."

Experience is good: there is nothing to equal it. Opinion is good, when nothing else is available. Suppose that experience consisted of watching a roof truss for 40 years, a truss that calculations showed to be weak. Such experience would doubtless say that the truss was perfectly safe. The truss the writer has in mind stood for 42 years and then collapsed. Suppose it consisted in watching a railroad girder span for 30 years, part of which time it had been loaded somewhat beyond its calculated capacity. The experience would say that the span was perfectly safe. The span the writer has in mind was in Pittsburgh; it failed after more than 30 years of service under usual conditions, slow speed of a locomotive not very heavy. The occurrence took place May 7, 1900, and the writer was on the ground before the police shoved the inquisitive back. By his calculation the unit stress was something less than 14,000 lbs. per square inch in tension on wrought iron, a little more than half of what Mr. Greiner considers perfectly safe—perhaps 40 per cent of overload above the designer's assumption.

A short time ago another fatal wreck occurred on a viaduct that had stood for more than 20 years. A box girder, woefully bad in detail, for it was hollow where there ought to have been stiffeners over the column, crushed down over the column. If a detail so abominable as this could stand 20 years of heavy loads and constant traffic, would not experience pronounce it good?

None of these cases were of wearing out from wasting of the metal. The weakness was original and was ascertainable. Calculation and logical designing were infinitely better than decades of experience and observation in determining the sufficiency of these structures.

"Numerous tests" are also good, especially when the tests are carried to a conclusion. A contractor tests his equipment again and again, and after lifting a given load hundreds of times a chain will break under the same or less load. Calculation will show him that the chain was overloaded every time it lifted that load, but it only failed at the last time. This is in perfect agreement with laboratory tests. A large number of repetitions of loads near the elastic limit of steel will eventually break a specimen. The steel is rendered brittle by excessive loads and finally breaks. Chain users understand this and heat or anneal their chains occasionally to toughen them. Bridge users never anneal their old ill-used bridges.

If the numerous tests referred to by Mr. Greiner have been published, it would be well to call attention to them and show how they furnish a basis for shattering all standards for bridge design, for if 26,000 lbs. per square inch is perfectly safe, why waste money by using 16,000 lbs.? If such units constitute sound engineering in railroad bridges, where unbalanced loads go hammering over uneven tracks, to spare the railroads the expense of replacing old and weak bridges, why not use even higher units in buildings and in bridges where automobiles with their gum shoes go quietly by?

The elastic limit is the practical ultimate strength of bridge members, both in tension and in compression. This statement admits of no controversy. It is established beyond question that stresses at or near (below) the elastic limit will, under repeated application, cause failure.

The yield point of wrought iron specimen tests is usually about 26,000 lbs. per square inch. This is the point where "drop of beam" is observed. The true elastic limit is very much less, probably in the neighborhood of 20,000 lbs. The writer has taken steel with a nominal yield point of 40,000 lbs. per square inch, and by testing it under a very slow speed has found it to have an elastic limit of about 32,000 lbs. per square inch.

What degree of safety would there be in a structure whose tension members are stressed 22,000 lbs. in wrought iron having an elastic limit of 20,000 lbs. The structure might stand it for years.

What degree of safety would there be in a structure whose tension members are stressed 24,000 to 32,000 lbs. per square inch (50 to 100 per cent overload) in steel whose elastic limit is 32,000 lbs., or perhaps less?

Compression members, by the American Railway Engineering Association Specifications, have the same units as tension members (reduced for column length). These would be subject to the same excess loading in a bridge. What do tests show in regard to compression members? The best set of tests extant because they are made on actual-size bridge members of good average shop work, are those made by the late Mr. C. P. Buchanan and published in "Engineering News," Dec. 26, 1907. Mr. Buchanan in this article gives the elastic limit or yield point of these several tests, 19 in all. The maximum loads are somewhat higher than the yield point or point of permanent set, in some cases the difference is but a few thousand pounds, in others the ultimate strength is about double the load at permanent set. It is the yield point that counts. It is under this load or less that the member would fail under a large number of repetitions.

Nine of the tests of wrought iron compression members showed a permanent set or yield point between 13,200 and 20,500 lbs. per square inch (average 16,200 lbs. per square inch). These members, however, failed in the end details. Two steel members also failed in the details, showing permanent set at 23,600 and 24,700 lbs. per square inch respectively.

Three wrought iron members failed in the body, showing permanent sets at 17,000, 17,200, and 20,150 lbs. per square inch, respectively. Five steel members failed in the body showing permanent set at loads of from 12,600 to 25,500 lbs. per square inch (average 19,700 lbs. per square inch). The average maximum load was 31,200 lbs. per square inch. Four columns had a ratio of slenderness of 46 and less, the fifth had a ratio of 97 (ultimate strength 27,790 lbs. per square inch).

All of these were members having a ratio of slenderness between 30 and 100, except one wrought iron member whose ratio was 120 and whose per-

manent set was 20,500 lbs. per square inch, the highest value of all the wrought iron members.

These steel members would probably all fill every requirement of the American Railway Engineering Association Specifications. They were ordinary standard bridge members, the kind Mr. Greiner and other engineers would subject with equanimity to 50 to 100 per cent overload.

What degree of safety would there be in a structure whose compression members are stressed 24,000 to 32,000 lbs. per square inch (reduced for column length) when those members have a yield point of 19,700 lbs. and an ultimate strength of 31,200 lbs. per square inch as shown by tests?

Six tests on well-made bridge members described by J. A. L. Waddell (in Trans. Am. Soc. C. E., Vol. LXIII, p. 250) show great regularity. In three of these bridge members l/r was 27, and the elastic limit was 28,800 lbs. per square inch—just about 100 per cent more than the safe load by the A. R. E. A. Specifications. In the other three members l/r was 81, and the elastic limit was 16,800 lbs. per square inch—60 per cent greater than the safe load. What would happen to these members under repeated applications of loads 50 to 122 per cent greater than that allowed by the above specifications? The average ultimate load of the first three members was 39,200 lbs. and of the other three 30,500 lbs. per square inch—less than three times the A. R. E. A. safe load. The old notion about a railroad bridge *being designed* with a factor of safety of four or five is a mistaken one. If any engineers still fancy that it has any meaning it would be well for them to revise their fancies.

Now where is there any room for opinion when bald facts bellow out the truth in such unmistakable terms?

Very truly yours,

EDWARD GODFREY.

The statements of this letter are not matters of opinion in any sense whatever. They are bald facts. Personal judgment and experience do not enter into them in any degree. They would be just as true if searched out and stated by an undergraduate as if they were the statement of a man who has had a mere twenty or thirty years of experience as a structural designer and specialist.

The force of an argument based on fact is in no way altered by the possession or lack of experience or first hand knowledge on the part of the one who states the argument, and no structure ever was a whit stronger because some self-styled authority pronounced it safe.

In the same issue of *Engineering and Contracting* Mr. Greiner replies. He does not take exception to the statement of a single fact as to the actual strength of bridge members in my letter, nor does he say that my letter misrepresents or misquotes his report. He does say that bridges and freight cars

can be loaded beyond their design capacity. This no engineer will deny. The question my letter brought up was the excessive amount of that overload as proven by tests.

Mr. Greiner says that my letter indicates that "he has not read my paper carefully or that he is not familiar with the subject which he endeavors to discuss." Again he says "I heartily agree with him, but if there are to be more laws they ought to be framed by someone familiar with the subject to which they relate." This kind of argument needs no comment.

CHAPTER IX

BRACING

In *Architecture and Building,* in 1913, a series of articles appeared which I wrote for that publication. They were on the subject of failures in building construction and their lessons. The article that appeared in the August number was on bracing. The substance of that article is repeated in this chapter.

Of equal importance with the actual strength or cross section of members of a structure is the bracing of its parts. Many failures of structures have been the result of lack of bracing, but strange to say, the lesson is not always learned. Many times the facts are perverted and the lesson obscured.

The need of bracing is equally great both in the parts of a building and in the building as a whole, and failures are frequent from either cause.

GENERAL NEED OF LATERAL STABILITY

It is not sufficient that bracing be supplied only where definite lateral forces exist, such as the bracing to resist wind stresses: there is a general need of lateral rigidity in any structure where compressive stresses are carried. The points of these members where they receive their loads, and, if they are slender, intermediate points must be held against lateral displacement. By observing these precautions very light structures may be made to carry safely very heavy loads; by ignoring them, very heavy structures may collapse from their own dead weight.

BRACING IN PARALLELOGRAMS NOT EFFECTIVE

An error very often made is to brace one weak member by connecting it to another equally weak. When this is done by a triangular system, that cannot change its shape without shortening or lengthening one or more of the braces, effective bracing can be accomplished; but where the system is in parallelograms, this is not the case. Two columns connected by a horizontal strut are not effectively braced, nor are they shortened in their effective length as compression members, for both columns may deflect horizontally without resistance from this strut.

KNEE BRACE TO WEAK STRUT NOT EFFECTIVE

In Fig. 1 the two columns at (a) have an effective length equal to A C. (It is assumed that the tops of the columns are held against horizontal displacement.) At (b) it would appear that the knee braces would shorten the effective length of the columns, but here is another error in design very often made. Unless the strut itself is capable of taking bending stress, these knee braces add but a trifle of stiffness to the columns. If the knee braces met at the middle of the strut, a triangular system would exist, and substantial rigidity would result.

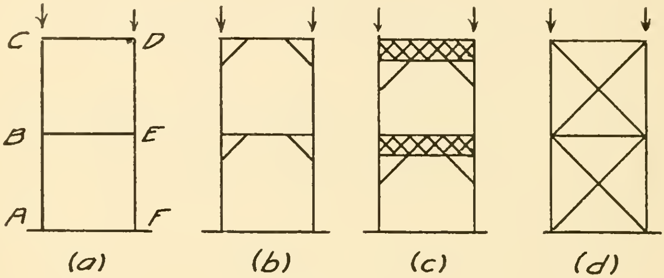


Fig. 1.

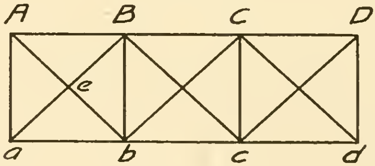


Fig. 2.

At (c), Fig. 1, with struts of considerable vertical depth, the knee braces would be effective. It is not to be forgotten, however, that the bending moment put into this strut by the knee braces must also be shared by the column which the strut and knee brace are meant to stiffen. Bracing such as this is often required where headroom and clearances must be maintained. It is an error often resulting in weak design to neglect the effect that such bracing has on the columns themselves. This sort of bracing is usually referred to as portal bracing. It is particularly useful in resisting active horizontal forces as in the wind bracing of tall buildings. Floor girders or wall girders may take the place of the strut, and gusset plates, stiffened on the edges with angles, may take the place of knee braces.

EFFECTIVE BRACING

At (d), Fig. 1, is shown the most effectual and economical form of bracing. The horizontal struts and the tension diagonals completely

relieve the columns of bending stresses at the ends and at the middle, so that the unsupported length is equal to A B or B C.

COMPRESSION MEMBERS MUST BE HELD IN ALL DIRECTIONS

An error sometimes made by experienced designers is to assume that because a compression member is connected to another member at its middle point it is prevented from buckling in all directions. To illustrate: The member aB of the truss in Fig. 2 is a compression member. The tension member Ab holds it against any movement in the direction of A or b. It does not, however, hold it against deflection in a direction perpendicular to the plane of the truss. In construction such as this the member aB should be deeper in the direction perpendicular to the truss than in the plane of the truss. The unsupported length should be considered as aB for the radius of gyration of the section which is normal to the plane of the truss.

If the foregoing simple rules and precautions were observed in designs, much weak and dangerous construction would be avoided and many failures averted.

COLUMNS MUST BE HELD UPRIGHT

Grandstands and timber frame-work often collapse. It takes a large compressive stress in a sound timber post to cause actual failure, when the post is held upright and not allowed to deflect. But it does not take much to pull over a row of posts that are not stiffened by diagonal braces. It can be set down as a general rule that when there is an extensive failure in a structure supported on posts or columns, it is the columns that have given way. Very few failures ever take place where the supporting columns are left standing upright. The two ways in which a series of columns may fail are by the individual columns crushing or buckling or by the series toppling over. Excepting where the columns are rodded concrete the latter is the usual if not universal way that large failures occur, and lack of diagonal bracing is the cause, or, what is equivalent, lack of ability of the columns to resist bending at floor levels.

An example of a failure of this sort occurred in Brooklyn in August, 1912, as described in *Engineering Record,* Aug. 10, 1912. A large roof collapsed because it was insecurely supported on wooden props placed on top of the steel beams and not steadied with diagonal braces.

TIMBER ARCHES COLLAPSE—NO DIAGONAL BRACING

A notable example of failure in timber work occurred in Spokane, Washington. This is described in *Concrete Engineering,* Sept. 19, 1910. A

set of timber arch ribs built as centering for a masonry or concrete arch toppled over. These arch ribs were apparently well built. They had substantial looking top and bottom chords joined by diagonal web members. Only the lower chords, however, had skewback supports at the ends. The seven arch ribs did not appear to have had a stick of diagonal bracing, and they were not even guyed to prevent swaying. They were joined by timber pieces square across but not by diagonal pieces, for which there was ample opportunity. The immense structure would probably have been perfectly safe, in spite of the error in lack of abutment for one of the arched chords, if the simple and elementary precaution had been observed of introducing some diagonal braces.

SOLITARY METAL CAGE BUILDING FAILURE—CAST IRON COLUMNS

Practically all of the great wrecks of buildings, excluding those of reinforced concrete (where rodded columns are primarily to blame) have been due to lack of effective bracing of the structures themselves either during or after erection. One large failure in New York was that of an apartment building of about twelve stories. The building had cast iron columns. (See *Engineering News,* March 10, 1904, for description of failure.) This is the one single failure of a metal cage building which has collapsed in ruins in spite of the existence in many such structures of poor design and inadequate size of members. Reinforced concrete failures, of rodded column installations, are numbered by the score, all because of the inadequacy of rodded columns.

A steel frame building, especially one which has no diagonal bracing, derives a large part of its stability from the ability of the columns to take bending, particularly at the floors. The columns are spliced with cover plates riveted on the sides. In just such manner does a tree resist the overturning force of the wind. (This, of course, applies to floors from the first up, as the heavy basement walls and first floor furnish substantial "ground" out of which the steel columns "grow.") Cast iron columns are not spliced with anything like the same efficiency as steel columns, since they are merely bolted with a few flange bolts. Furthermore, cast iron does not have the toughness that steel has, and cannot resist overstrain nor distortion in any degree. The error in using cast iron columns in a high building or where the columns are depended upon to add rigidity to the building by resisting bending is apparent.

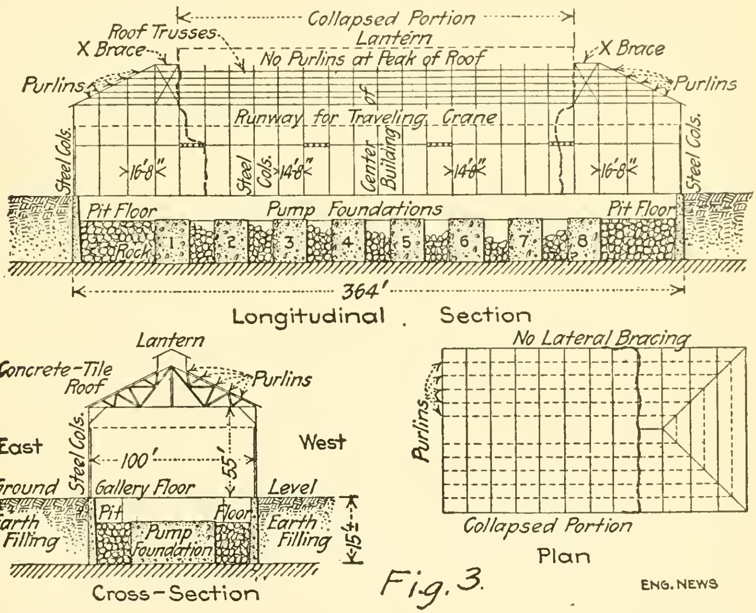
ROW OF TRUSSES MUST BE BRACED AGAINST SWAY

A failure similar to that of the falling over of a row or set of columns in one general direction because of the lack of bracing between the

columns is the collapse of a row of trusses due to the lack of diagonal bracing to hold the trusses upright. A number of very serious failures have taken place because of lack of provision against the toppling over of rows of trusses.

The great coliseum building in Chicago was being erected and a long row of large trusses was standing without being braced. The trusses fell over in ruins. The same building was being reconstructed and a long row of trusses had been erected. Attempt was made to slide along a beam by means of a line attached to one of the trusses, and the entire line of great trusses was pulled down the second time.

A theater building in Brooklyn, N. Y., failed in a similar manner. The line of trusses fell over because there was nothing to hold them upright.



Outlines of Porter Ave. Pumping Station, Buffalo, N. Y., Showing Portion Which Collapsed June 30, 1911

This failure is described in *Engineering News-Record*, Dec. 8, 1921. A number of explanations were published as to why this structure failed. One of these blamed the brick pilasters, another blamed a column supporting the end of a heavy truss carrying the line of trusses. In a letter of mine published in *Engineering News-Record*, Jan. 5, 1922, p. 33, the big error of the design is pointed out as being the absence of any kind of brace to hold the peaks of the line of trusses from flopping over.

In these several examples of failures of rows of trusses purlins have

been in place connecting the trusses together, but the construction has lacked diagonal braces to prevent the whole line of trusses from swaying in the same direction.

ENDS OF COMPRESSION MEMBERS MUST BE HELD

A failure of still another type was that of the Buffalo Pumping Station which is described in *Engineering News*, July 6, 1911. This is one of the rare examples where the real cause of the failure is stated in a published report upon the same. (See *Cement Age*, October, 1911, p. 171.) The roof was 360 feet long and 100 feet wide. Nearly all of the roof collapsed, with loss of life, all of this loss was due to neglect of the simple precaution of bracing the ends of compression members. Fifteen 100-ft. trusses of this roof collapsed because there was no line of struts at the peaks of the roof trusses. The peaks of the main roof trusses were covered by a lantern or monitor about 16 feet wide, and at the point where the two inclined chords met, and where two other main members centered, there was nothing whatever to give lateral stability.

The construction is illustrated in Fig. 3. There is nothing to prevent the chords from swaying at the peaks of the main trusses. This is just what happened, and as a natural consequence every truss not held at the peak collapsed. A few dollars worth of steel would have saved the building. This could have been in the shape of a line of struts from peak to peak or even as many rods. The ends of the building had hipped trusses. The two trusses (near the ends) which supported the hip trusses remained standing, as well as the next two adjacent trusses, which had struts connecting to the peaks.

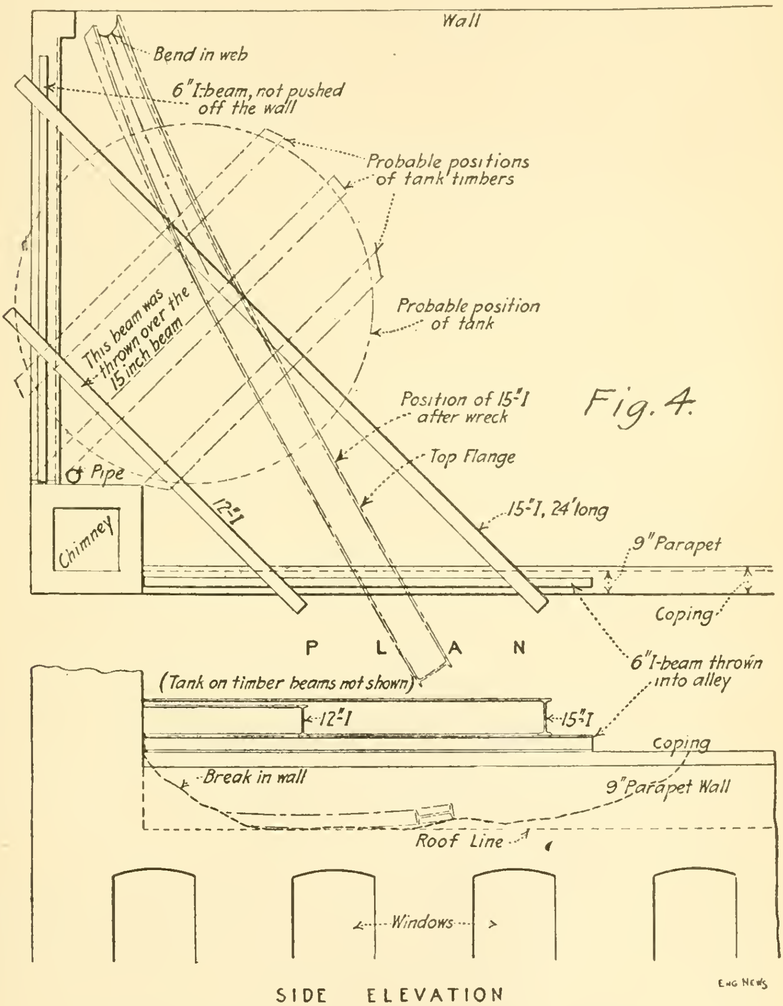
By simply holding the ends of the compression chords of these trusses in place the strength of the whole truss would have been increased several hundred per cent.

Something was needed to steady the ends of the compression members. This is one of the essential points to consider in bracing a structure. The other is to steady the structure as a whole against wind or other lateral forces. In this structure there was an example of main compression members connecting to a gusset plate which was in no wise held from lateral movement except by what stiffness was contributed by the compression members themselves.

A large crane at Panama failed from a cause exactly similar to that which caused the Buffalo Pumping Station roof to collapse. This failure is described in the chapter on Equipment.

The ends of compression members, both those of trusses and the top flanges of girders, must be held against lateral displacement or else the structure is very greatly weakened. This was illustrated in laboratory tests made some years ago on I-beams. The tests were published in the Proceed-

ings of the American Society for Testing Materials, in 1909. They were very fully quoted and generally commented upon in Engineering papers in 1909. I pointed out in several published letters that these tests were misleading because the compression flanges of the beams were not steadied at the ends. Very low elastic limits and ultimate strengths were obtained. These



Elevation and Plan of Roof Water-tank Support

low results were attributed, by the author of the paper, to poor material in the beams. However, there is nothing in the beam tests to substantiate this. In tests made on beams formed of tin plate, described in *Engineering News*, Jan. 6, 1910, I found an increase of 129 per cent in ultimate strength by the

mere addition of end stiffeners on a beam to prevent the ends from keeling over. In another set of tests wooden blocks fitted squarely against the flanges gave a still greater increase of ultimate carrying capacity. In all of these tests the unstiffened beam failed by keeling over of the ends, in opposite direction at the two supports, causing a large twist in the web.

TOP FLANGES OF BEAMS AND GIRDERS MUST BE STAYED

A failure illustrated in Fig. 4 is that of a tank support on the roof of a building. This is described in *Engineering News*, Aug. 29, 1912. The faulty features of this design are so numerous that they will be given here for the lesson they enforce. In the first place the main supporting beams rested on parapet walls only 9 inches thick, of brick laid in lime mortar. No such walls should be compelled to carry heavy concentrated loads. On the thin stone coping of this wall there rested a distributing beam. This was a single 6-in. I-beam. It should have been a double beam with separators. A single beam used thus is not properly braced against lateral displacement. The beams supporting the tank were laid diagonally across the corner of the building on the two six-inch I-beams. One of these supporting beams was a 12-in. beam. The other was a 15-in. beam, 24 ft. long. Neither had any bracing from end to end, but they stood up free and supported the weight of a heavy tank. The larger beam, being overstressed and lacking bracing, keeled over at the end and dropped the tank with fatal results. Another fault with this construction was that the tank was so placed as to throw nearly all of its weight on one of the beams.

Pony trusses, that is low bridge trusses with no overhead bracing, are frequently built with insufficient bracing to hold the ends of the top chord members in line. The whole carrying capacity of the bridge is thus very greatly reduced. An 85-ft. pony truss span failed under its own weight, because there was no bracing of any kind in the top chord. (See *Engineering News*, March 13, 1913.) Another failed because it had only a single brace at mid-span. The gusset plates formed a toggle at the hip. (See *Engineering News*, Nov. 6, 1913.)

DISASTROUS RESULTS OF OMITTING DIAGONAL BRACING

In *Engineering Record*, Sept. 21, 1901, page 281, there is a long description of the failure of a plate girder span 147½ feet long which was brought into requisition by the military department in France to take the place of a washed out bridge. This girder span had no diagonal bracing, either lateral or sway. It fell, of course, under a test load of a small locomotive. In great detail the account tells of the test and the failure, even down to the particular rivets that failed first. It would have been more illuminating if it had told why the designer was not arrested and jailed before the test was made.

Europe has furnished us plans of plate girders with great square open holes where there should be a continuous web plate or diagonals, and this kind of design is pointed to by advocates of the stirrup in reinforced concrete to sustain their contentions.

It is an elementary principle of mechanics that a rectangular system is not an efficient system of bracing nor of truss action, and in many cases it is positively weak and dangerous. It is only in portal action and in cases where bending stresses are fully taken care of that a rectangular system is permissible or safe.

BRACING NEEDED AT ENDS OF THROUGH GIRDERS

Through girder spans are sometimes built with but little bracing to steady the top flanges at the ends of the girders. This is particularly true of the forward girder in a skew span, where the end stiffeners are sometimes the only means of preventing sway of the top flange of that girder. There should be a gusset plate connected to the shoe or preferably connected to an end strut.

CAPACITY OF GIRDERS REDUCED BY LACK OF BRACING

The bracing of compression chords and flanges at intermediate points as well as at the ends is of great importance. A very common fault in the design of girders in crane runways is the lack of lateral stiffness in the top flanges of the girders. Designs are frequently made where the nominal capacities of beams or girders are used, though these nominal capacities are based on flanges that are held laterally at intervals only a fraction of the span length, and the top flanges of the girders are lacking in lateral bracing. Crane girders need special provision for lateral rigidity because of the active lateral forces exerted by the crane while loads are being lifted and moved about.

FIRST FAILURE OF THE QUEBEC BRIDGE

The most gigantic piece of book engineering of modern times is summed up in the mass of opinions as to what made the Quebec Bridge fail and fall. With eyes totally blind to the menace of a great traveler covering the space of a sixteen story sky-scraper, and bearing a million pounds of steel at its top, with no sign of bracing between its side bents, men groped around in books and theory for some justification of the hypothesis that two chord members, out of twenty or thirty similar members equally or more highly stressed decided to let go at the same instant. And what does this alleged justification rest on?—model tests the results of which were clipped down 17½ per cent for supposed friction and error of a machine *that has*

since been pronounced by Government authorities to be practically correct.

Books say little on the necessity of bracing high structures. They say much on the strength of compression members. Theory of lattice bars can be stretched to great length. Why did the Quebec Bridge fall far to the right out where the traveler was located? What pushed to the right the two bottom chord members and the stringers at the bottom end of the first three stiff members of the anchor arm? Why did the cantilever arm show a progressive collapse and not fall as a whole, if the bottom chords of the anchor arm were the point of initial failure? Why were the strength and bracing of the traveler not looked into in the investigation? Why was my theory, published in the *Engineering Record*, Sept. 21, 1907, covering these points, totally ignored in the investigation? That theory has never been challenged or contradicted, so far as I can learn. A theory that makes plain every published fact about the wreck ought at least to be explained away, if it is not adopted.

A MINORITY REPORT ON THE QUEBEC BRIDGE DISASTER

By the Author

Published in 1908

I have no authority or right to make any kind of report on the subject of the Quebec Bridge disaster other than the right of an American citizen to express his opinion and a civil engineer to defend the profession to which he belongs against self-inflicted abasement, as it has sat in the ashes for a year and heaped dust upon its own head.

I propose three theses, as follows:

(1) The civil engineering profession has no reason whatever to hang its head on account of the Quebec Bridge disaster.

(2) The cause of the disaster is the common cause of practically all of the great structural wrecks recorded, and it can be stated in three words, namely: "insufficient diagonal bracing."

(3) The one great lesson of the disaster has been almost totally ignored by writers upon the subject in the voluminous articles and report that have been issued.

I shall take up these theses ad seriatim. I say that the civil engineering profession has no reason to hang its head on account of the Quebec Bridge disaster. Until it can be shown that the profession would have approved the details of the design and the manner of erection of the bridge this thesis holds, and if it can be shown that the profession would not have approved the design or the manner of erection, they have a right as a class to disclaim all responsibility for the disaster.

It might seem difficult to go back of the disaster and determine what engineers would have approved previous to this event, which is sup-

posed to have upset their existing ideas of the strength of large bridge members and of great structures; but the case is not so difficult as it appears. Engineers had and have standards by which they judge the strength of a structure and of its parts; and if it is shown that these standards were violated in the Quebec Bridge design, the case against the profession falls. The standards referred to are the specifications for steel bridges in common use and those adopted by various railroads for their own structures. Every structural engineer is familiar with the requirements of many of these specifications. It does not take a deep study of the unit stresses of the Quebec Bridge and any of the standard specifications whatever to see that this bridge was designed for unit stresses far greater than would be allowed in the cheapest kind of highway bridge work.

Designed on the basis of the Pennsylvania Lines Specifications the bridge at the time of the collapse had unit stresses about 30 per cent greater than those specifications would allow, though subject to only a part of its dead load stress. By the same standard, if we assume a live load stress one half of the dead load, some members in the finished structure would have required nearly 100 per cent more area. By Cooper's Specifications and those of the M. of W. Asso., the same members would have required about 50 per cent more area. The engineering profession is by this documentary evidence cleared of all responsibility for the weakness of this bridge in the matter of sectional area of its members, and that they were weak is unquestioned.

The bridge was constructed of ordinary structural steel, and the tensile and compressive strength of ordinary structural steel was well known before this failure. It was known then and is known now that structural steel in compression members of even moderate length will not stand an ultimate unit stress nearly so great as the same steel in tension. If this bridge had ever reached the stage of service, the unit stresses in the compression chords would have been far in excess of those in the tension chords. It is true that some standard specifications allow the same unit stress in short columns that they allow in tension members. This is indefensible, but it is not a subject under discussion here.

One of the responsible engineers said in his testimony before the Royal Commission, "The changes in unit stresses for compression members carried them out of the field of past experience in bridge construction and detailing, and did not follow usual practice." The designing engineer said, "There were no precedents for designing compression members of this magnitude. Tests made on small pieces do not furnish adequate information for members of many times their size." The force of this assertion is entirely nullified by his next answer, namely,

"The largest compression member designed by me had 240 sq. in., and the unit stress was 14,000 lbs.," in view of the fact that the unit stress in the Quebec Bridge chords of about three times this area was nearly double this amount.

It is admitted that the exact ultimate strength of a compression member, large or small, short or long, cannot be predicted, because it depends largely upon imperfections and this in a manner not approached in tension members. However a bridge should not be built to see how close to the ultimate the stress can be carried without failure. Every structural engineer knows that what is called a factor of safety must be applied in all members of a truss, so that the stresses will be well below those that will cause distortion. The great cost of a bridge, if built according to well-established rules for safety, is no justification for its being built entirely outside of and in violation of those rules.

It is clear from the further evidence above referred to that engineers in general cannot be blamed for the high unit stresses used in this bridge.

As to lattice bars, here again custom was departed from. The lattice bars in these heavy chords were in no sense proportioned to the magnitude of the members, using as a basis common practice of engineers as exhibited in countless bridges doing service throughout the world. The latticing of these chord members was very weak. The latticing of the vertical posts of the trusses, considering the work it had to do, was immensely weaker: this is a weakness not even hinted at in the voluminous report and writings on this bridge, that are supposed to point out the lessons to be learned from the failure. The weakness just mentioned will be referred to again in this paper.

The meat of the Royal Commission's findings is contained in the following, quoted from their report:

"As a conclusion reached from the evidence and from our own studies and tests, we are satisfied that the bridge fell because the latticing of the lower chords near the main pier was too weak to carry the stresses to which it was subjected, but we also believe that the amount of those lattice stresses is determined by the deviation of the lines of center pressure, from the axes of the chords, and this deviation is largely affected by the conditions at the ends of the chords. We must therefore conclude that although the lower chords 9-L and 9-R anchor arm, which, in our judgment, failed from weakness of latticing, the stresses that caused the failure were to some extent due to the weak end details of the chords, and to the looseness or absence of the splice plates, arising partly from the necessities of the method of erection adopted, and partly from a failure to appreciate the delicacy of the joints, and the care with which they should be handled and watched during erection. We conclude from our tests that owing to the weakness of the latticing, the

chords were dangerously weak in the body for the duty they would be called upon to do. We have no evidence to show that they would have actually failed under working conditions had they been axially loaded and not subject to transverse stresses arising from weak end details and loose connections."

Granting for the sake of argument that this was the true and sole cause of the collapse, what is there in it to condemn the engineering profession? What ground is there here for such assertions as the following, quoted from a recent editorial in an engineering periodical of high standing? "Unfortunately the disclosures of the past year indicate with a clear certainty that compression members must be more heavily built than has heretofore been thought necessary."

But to anticipate my second thesis:—while it is not disputed that the most important members of this bridge were woefully lacking in cross section and in diagonal bracing of their parts together (the latticing), these weaknesses were merely accentuated by the failure and were not the cause. The starting point of the failure, and the real cause, lay in absence of diagonal bracing in the immense traveler used in erection. The toppling over of this giant gantry pulled the bridge over and set off all of the weak and highly stressed parts, as a much stronger structure might have been wrecked, though probably with less ignominious failure.

Unfortunately the case here in favor of the engineering profession is weak. In all the books on structural design the most inconspicuous lesson is that tremendously important one of the necessity of bracing in all structures. The result is the sad fact that nearly every structural wreck of any consequence is due to lack of diagonal bracing.

A great auditorium in a great city was being put up. A large portion of the truss work was erected. In the quiet of the night this immense mass of steel work collapsed. The cause was *insufficient diagonal bracing.* Let us see if the lesson was learned. The same structure was being re-built. A large part of the steel work was erected. The erectors took hold of a beam lying on the ground to "snake" it along by a line attached to one of the trusses. The entire line of trusses was pulled over like a row of blocks, and another great wreck was recorded, due to *insufficient bracing during erection.*

On June 27th, 1906, I wrote to an engineering paper calling attention to the lack of diagonal bracing in the Quebec Bridge erection traveler and pointing out the menace of using wire ropes as braces, as the design apparently contemplated, because a wire rope of equal tensile strength will stretch many times as much as a rod. I referred to a wreck which occurred more than two years previously and mentioned its probable cause as being the bracing of the traveler with wire ropes; as this had never been hinted at in any engineering publication. A few days after

writing I received from an associate editor of the journal a letter, from which I quote as follows: "This happened so long ago that it does not seem desirable to give prominence of it in our columns at this time."

While it is true that risks are often taken in the erection of structures in the matter of bracing, it is also true that it is the rule to take precautionary measures in this matter; and the risks that are taken are not sins to be blamed on the profession in general, but on the reckless ones who take them.

To proceed to my second thesis, I propose to show, by the facts brought out by the Royal Commission in their report, that this (to be) greatest of all structures failed from the simplest of all causes. No spider would build a web without bracing it diagonally. I have already mentioned the fact that the structure itself was weak by reason of inconsistent latticing of the great compression members, and that the traveler was lacking in the same essential of safe construction, namely, diagonal bracing. I will take up first the Royal Commissions conclusions and examine their plausibility.

The Royal Commission's report sets forth the theory that two bottom chord members of the anchor arm, namely A9L and A9R, failed practically simultaneously, and that the initial failure was in these members. One of these members, the left hand one, had been observed to be bowed about two inches in the 57 feet of its length. No stress whatever is laid upon this condition by the Commission, and there is a most potent reason for ignoring it; for there is absolutely no getting away from the conclusion that if this left-hand member had failed first, the whole structure would have fallen to the left. It fell to the right.

It is hard to conceive what sort of sympathy these two members, out of 20 or 30 similarly designed members, some of which were more heavily stressed, could have existed to cause them at practically the same instant to let go. Such an occurrence is so improbable as to be unbelievable.

The Royal Commission lays the weakness of the chord members in the end gussets. But the failure of these members was not in the ends: it was about at third points. It is true that they say that the weakness of end details caused excessive stress in the lattice bars, but it is also true that their conclusion as to the weakness of the latticing was drawn from tests; in these, weakness of end details would not enter. They say that they have no evidence to show that the chords would have actually failed under working conditions; that is, if the gusset plates had been fully riveted up, and the bridge had reached completion, and the stresses in the chords were central, there is nothing to prove that the 28,000 lbs. per sq. in. of stress in these chords would have caused failure. This is the most remarkable and significant statement in the report. I am inclined to agree with it, though the margin of safety of the structure would have been very narrow; but what of the model tests that purport to show that the chords would fail under 18,000 lbs. per sq. in. of *central* loading?

Many engineers say that the bow which was observed in A9L increased until failure occurred. Laying aside the improbability of this, as mentioned heretofore, (since it would throw the bridge to the left), this sort of failure is totally at variance with the facts. If the member started to bow in failing, it would continue to bow and would double up on itself. But it bent instead in S-shape. This would necessitate reversing the bow about one-third of the length of the member from the end, a thing utterly impossible. The Commission wisely dropped the theory of failure by bowing of these members.

Let us see what would happen if the two bottom chord members A9 should fail at once. The stresses in the cantilever arm would be the same as before the failure of these chords. At the foot of the main posts there would be a horizontal thrust of ten or twelve million pounds on each shoe. This tremendous force would act immediately, and it would of necessity either meet with a reaction or cause motion. Before this hypothetical failure the bottom chord of the anchor arm met the reaction, but with this line of chords crippled their power to meet the reaction would be negligible before such a force. The only thing left to meet the reaction is the top of the pier. Twenty or more millions of pounds of horizontal force on the top of the pier could not possibly be resisted by the pier, and yet the facts show that not a joint of the pier was disturbed, and its distance from the abutment remains exactly the same as before the failure. If then this great thrust was not met with a reaction, there would have been motion and motion of a sudden nature. A force of this magnitude suddenly relieved of resistance would not result in a slow collapse of the structure, but would immediately kick the supporting shoe off the pier and drop the cantilever span as a whole. All witnesses agree that there was a gradual sinking of the cantilever, and the shoe did not slip off the pier until the cantilever was well down toward the water surface. The shoes dropped just inside of the pier and the greater part of the main posts over the pier fell riverward, with the finials *pointing to the right.*

A theory was advanced by me in the *Engineering Record,* September 21, 1907, which I quote below:

Sir:—I beg to submit a theory of the Quebec Bridge disaster that accounts for every phase of the disaster, so far as the facts have been made public. The theory thus far given the greatest prominence is entirely inconsistent with the facts for the following reasons. (This theory is that a bottom chord member, A9L, on the left truss in the anchor arm failed and brought down the structure.) In the first place it is unbelievable that a member of the size of the one under suspicion, with the intensity of strain it sustained, could have failed under a steady load. In the second place, initial failure of a bottom chord member on the left truss would have thrown the whole structure to the left and not to the right as it actually fell. In the third place, initial

failure of the bottom chord of the anchor arm would have thrown an enormous thrust at the foot of the tower and pushed it toward shore instead of allowing the greater part of the tower to fall riverward. In the fourth place, if the failure were the result of the continuation of the weakness observed in bottom chord member A9L, we should look for that member to fail by bowing, since it was a partial bow that was observed. In the general collapse, however, this member took an S shape.

My theory is that the traveler, which was about twice as high as the truss where it was standing, was either pulled over by accident or fell over due to insufficient cross bracing, and that this fall to the right threw the whole top chord system to that side, crushing the vertical posts and thus causing cumulative failure of the entire superstructure. The pulling over of the erection gantry or traveler may have been done by the workmen in bracing the traveler for the night. It is customary to take the falls from one side of a traveler and attach the block to some part of the other side of the structure, then vice versa, forming X-bracing. The pulling on one such diagonal line could easily start the traveler to fall. The truss posts could not resist the fall.

The reasons I have for believing that the whole cause of the failure was due to the traveler are as follows:

(1) The traveler was being dismantled, and, though 300 tons of its weight had been removed, 800 tons remained. If only 500 tons of this were in the top structure, there would be a force of about five tons at the top chord of truss for every foot that the side bents were out of plumb.

(2) The traveler had little or no lateral bracing. If it was braced, it must have been with wire ropes, which in any event by reason of their extensibility would allow large stretch and large swaying.

(3) The vertical posts, excepting the main tower post, had little stiffness laterally to resist such a force as this traveler would produce. The single lacing was in planes normal to the truss, making the posts exceptionally weak in that direction.

(4) A blow from the traveler, such as its falling to one side would produce, would cause just such a failure as the one that occurred. It would deflect the top chord, with the sway bracing, to one side, and the weakest part, namely, the part of the posts beside the roadway opening, would bend. This would allow the entire upper system to come down.

(5) This theory would agree, with what little has been published as to the observation of men at the bridge. They say no swaying of the structure was observed. This would be expected on account of the stiffness of the lateral system at floor level. They say that the floor of the anchor arm sunk and they had to run up hill. This would be the case if the upper structure should come down: the lower chord as an arch would not stand the weight of the floor. If the lower chord failed first on one side and in one member, there would be swaying of the floor, because of the unsymmetrical failure.

(6) The S shape assumed by A9L, the lower chord member that is supposed by some engineers to have been the cause of failure, fits exactly in this theory. Your account states that this S was in both members A9. The lateral thrust to the right would be communicated down the first main diagonal and the first main post in the anchor arm, at the foot of which one end of A9L was located; and this end of A9L would be pushed to the right in forming the S, just as it was found to be. This lateral push was even enough to overcome the observed bow of which so much is made and reverse half of it.

(7) The failure must have been something that affected both trusses alike, otherwise the almost vertical drop is inexplicable. This would be the case with a lateral force exerted against the top chord; for as soon as the vertical posts, which were under heavy stress, would bend to one side enough to fail, everything else would give way almost at once, and every part of each truss would be affected just alike. The sway bracing between posts above the overhead struts and below the floor would hold the posts vertical in these portions until the force of the fall would crush the posts.

There is nothing mysterious about the above theory to account for the failure, and it does not carry with it the suspicion that all other large structures in which there are heavy compression members may be on the point of failure. The failure is simply brought into the class with about all other great erection failures, and is due to an omission to provide sufficient unyielding sway bracing. Yours very truly,

EDWARD GODFREY.

Pittsburg, Sept. 14, 1907.

Since the above letter was published it has been brought out that the structure out where the traveler was located was thrown far to the right. It has been observed that not only the two bottom chords of panel 9 are bent in the three-fold double, but two of the stringers of the same panel have the same characteristic failure, showing unmistakably that something took hold of the shore end of this panel and pushed it to the right. These two striking features of the wreck have not been explained by the Royal Commission in their long report. The matter of erection as regards the instability of the unbraced traveler was not even touched upon by them. My letter was public long before they began their investigation.

As to my third thesis: the lesson referred to as having been lost is that of the need of bracing during erection of any structure. There are other lessons to be drawn from the occurrence. One is that class work is immeasurably better than one-man work in structural engineering, as in many other things. If the engineers of this bridge had observed the consensus of opinion, as expressed in existing structures, the design would have been more substantial. If they had listened to the shopmen who perceived the weakness of the lattice bars of the chords, these would have been made stiffer.

In the matter of eye-bars the lesson is an encouraging one. Of over 700 eye-bars examined, only one was broken, and this was manifestly due to the fall and not to stress. The absolute reliability of upset forged eye-bars as tension members under reasonable unit stresses was demonstrated.

In a drawing accompanying this paper I have endeavored to show how this failure occurred. If there is anything regarding the failure that does not fit exactly into this theory, I have not heard of it.

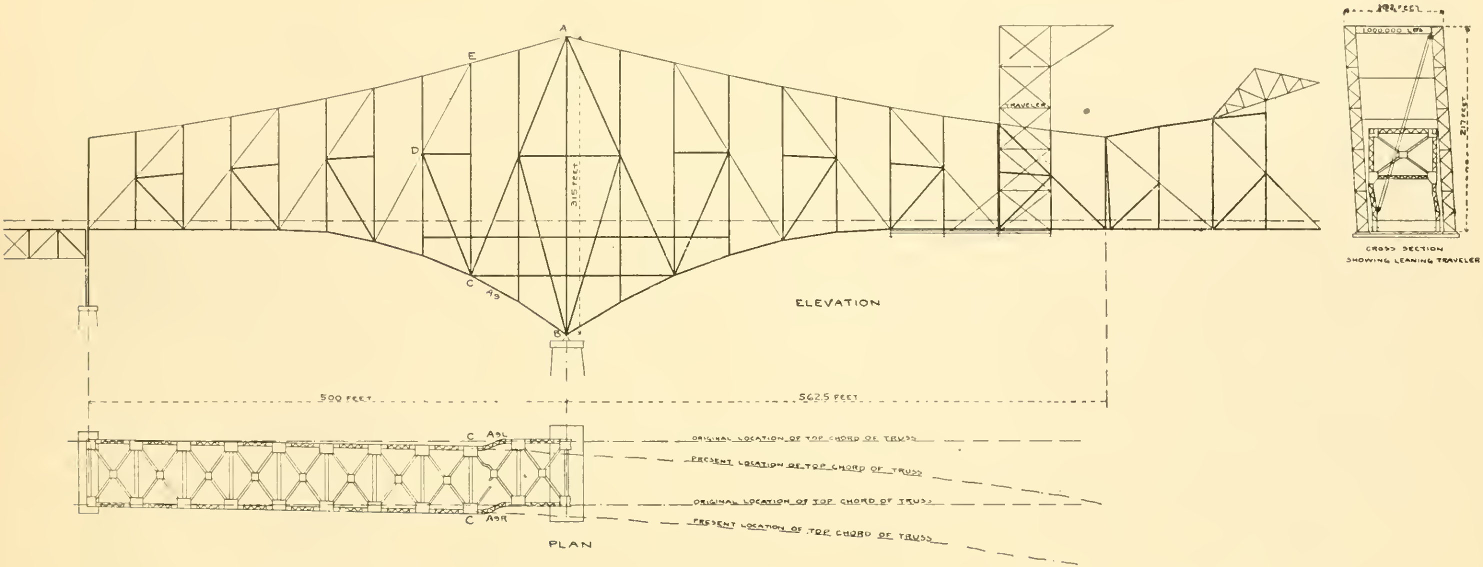

A
E
D
C
A9
B
315 FEET
TRAVELER
ELEVATION
500 FEET
562.5 FEET
C
A9L
C
A9R
ORIGINAL LOCATION OF TOP CHORD OF TRUSS
PRESENT LOCATION OF TOP CHORD OF TRUSS
ORIGINAL LOCATION OF TOP CHORD OF TRUSS
PRESENT LOCATION OF TOP CHORD OF TRUSS
PLAN
1,000,000 LBS
217 FEET
CROSS SECTION
SHOWING LEANING TRAVELER

Fig. 4

CHAPTER X

TANKS

There are several distinct causes of failures of tanks. One class of failures is due to the designing of conical bottom tanks without regard to the stresses at the junction of the bottom and the vertical sides. As books are practically silent on this point, designers are largely ignorant of the existence, importance, and magnitude of these stresses.

CONICAL BOTTOM TANK COLLAPSES

In 1901 a large water tank at Fairhaven, Mass., collapsed. The tank proper was 35 ft. in diameter and 50 ft. high. The bottom was in the form of an inverted cone for two circumferential courses of plates, then changed to a spherical form composed of a single plate. The total depth of the bottom was about 12 ft. (See *Engineering News,* Nov. 21, 1901.)

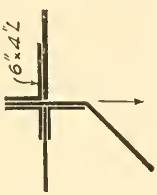


Fig. 1.

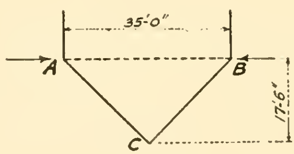


Fig. 2.

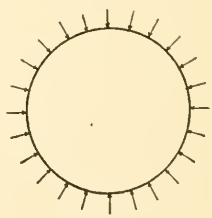


Fig. 3.

Figs. 1, 2 and 3 are sketches published in *Engineering News,* Jan. 2, 1902, in connection with a letter which I wrote to the editor. The following quoted from that letter is believed to be a complete explanation for the entire behavior of the tank and its collapse.

"It seems to be the prevailing impression that the bottom was subjected to annular tension. I think the following will show that it was really subjected to little or no tension, and this is the reason why it failed.

"Suppose the bottom were conical in shape throughout, instead of having a spherical-shaped terminal piece, and the dimensions were as indicated in the accompanying diagrams, the slope of the bottom being 45 degrees. This will approximate the true conditions, and will serve to illustrate the point in question. Assume the tank to be filled to a depth of 40 ft. That is, that a cylinder of water 40 ft. high and 35 ft. in diameter is pressing on the bottom, and that the unit pressure for the entire bottom is the same. This,

of course, is not quite true, for the pressure at the greater depth is greater, but the assumption will simplify the calculations.

"The weight of this cylinder of water is about 2,405,000 lbs. This entire weight rests on the conical bottom and must be transmitted to the support through radial tension in the direction of the elements of this cone. Since these elements are inclined to the horizontal at 45 degrees, the vertical and horizontal components will be the same. The vertical component is the weight of water or 2,405,000 lbs. This is the amount, then, of radial pressure applied around the rim as indicated in Fig. 3. By referring to Fig. 1 it is seen that there is nothing of any consequence to take this radial force. The 6x4-in. angle would take but a small part of it. It must therefore go into the conical bottom itself and produce an annular compression. The radial pressure on the periphery would be about 21,900 lbs. per ft. The equivalent pressure on the diameter is then 21,900x35, or 766,500 lbs.

"But there is a pressure acting in the opposite direction to offset this, namely, the pressure outward on the conical bottom itself. On the diameter this will be the area of the triangle A B C times the pressure of water at 40 ft. depth, or 35x17.5x40x62.5x½=765,600.

"The resulting radial tension is thus about nil. This makes the conical bottom in effect a flat plate, since it lacks the radial tension to hold it in shape. The stresses are those that would produce buckling and the local outward pressure that would tend to bulge out the bottom. Unstable equilibrium and the springing of the joints in the bottom would be the most natural consequence. The point of initial failure would naturally be one of the radial seams in the bottom. After this had bulged out the annular compression in the bottom, which supported the weight of the water, would take another course and immediately be turned into tension in the direction of the elements of the conical bottom. This would unseat the tank from its support, since it was not attached to the circular girder. The ripping of the joint of the conical bottom and the vertical sides would be inevitable. (All of this is just what happened in the failure of this tank.—Author.)

"If there had been a horizontal circular girder in the plane of the top flange of the circular girder used, of sufficient strength to take the annular compression referred to and attached to the flanged plate of the conical bottom with enough rivets to take 21,900 lbs. per ft., the conical bottom would be left to perform its proper function and would be in tension only. If this had been the case, the tank would probably be standing today."

MATHEMATICS NOT A SUBSTITUTE FOR STEEL

Provision for the annular compression at the junction of the cylindrical side of a tank and the conical or segmental bottom is of the utmost importance, and yet engineering works lay so little emphasis upon it that it is difficult to

find any clear statement of it. In reviewing a book I made the following statements in *Engineering News,* July 18, 1907: "On page 137 Professor Ketchum recognizes the annular compression at the junction of the shell and conical bottom of a bin, due to the weight of the contents of the bin hanging on the bottom sheets in their inclined position, giving an inward thrust which must be resisted at the corner. He, however, puts this down as a stress in the circular girder supporting the tank. Now this cannot be transmitted to the circular girder, unless the girder is continuously connected to the tank; and if the girder is so connected to the tank, the whole shell of the tank is a circular girder; and the principal office of this nominal girder is to take the annular compression and the small bottom flange stress of the shell as a circular girder. It is just a trifle absurd to give expressions to the fifth decimal place for the bending moments in a circular girder which supports a circular girder directly over it (the tank shell) that is immensely stiffer than itself. The vital point in the strength of a tank with a conical bottom or with a segmental bottom is this annular compression at the junction of the shell and bottom. If there is not a circular compression member rigidly and continuously attached to the tank, it is faulty in design and liable to failure. The circular girder, as a girder, is of no importance; it merely acts as a connecting medium to the columns. It is the overlooking of such incidental and apparently trivial points in design that gives rise to more failures than anything else. This annular compression is merely mentioned, while more than four pages are taken up with a discussion of the stresses in a circular girder based on a purely arbitrary assumption. A circular girder could be made that would be perfectly safe with a hinge where this discussion finds a maximum bending moment."

It might be added that a tank, without reinforcement at the corner where the bottom joins the shell, could be placed on that author's circular girder, with its large resisting moment and heavy top flange; and it could readily dinge in at the corner and slip off the girder. In spite of the nice stresses in the circular girder, another tank well reinforced at the "corner" could be set up on posts *without any circular girder and be perfectly safe.*

The book referred to was sent to me, in its second edition, for review. Not one syllable was changed from the first edition in these and other matters. When this fact was pointed out, in the second review, the author of the book replied that this was a rare bit of humor.

A failure which was from the same cause as that treated above occurred in a concrete grain bin at Ft. William, Ont. It is described in *Engineering Record,* Dec. 10, 1910, and in *Engineering News,* Jan. 19, 1911. Here an intermediate conical steel bottom gave way and in dropping tore a hole out of the tile shell. "Hasty design" is set down as the cause of the failure. The conical bottom was simply a flanged steel plate with the horizontal portion laid in the mortar joint between the blocks.

The lesson of these failures is simply that tanks of this sort must either be avoided by building the bottoms spherical or flat, or that a stiffening or compression member must be built at the corner. A horizontal curved lattice member, acting at the same time as a foot walk could be employed to overcome the weakness. In a small tank an angle at the corner may be sufficient. In the intermediate bottom in the grain bin a curved angle or channel could have been riveted to the plate and laid on the wall.

In *"Industrial Engineering and Engineering Digest,"* November, 1910, p. 373, there is shown a large water-treatment tank with an intermediate conical bottom. This is simply flanged up and riveted to the cylindrical shell with no provision to take the compression at the junction. This is the same error in design exhibited in the two examples which failed.

TANK WITH BATTERED POSTS PUNCHED IN BY HORIZONTAL FORCE

In 1907 a water tank on a tower 110 ft. high showed signs of weakness and incipient failure. This tank was 22 ft. in diameter and 30 ft. high in the cylindrical portion. The bottom was hemispherical. The tower had four legs which battered about 11 ft. in their height. The legs of the tower were riveted to the side of the tank. There was a light foot walk around the tank at the top of the tower legs. The signs of failure in this tank consisted in the tower legs being pushed in at the top and causing waves in the foot walk and being bowed out below the first line of tower struts below the tank. The cause of the weakness, which would probably soon have resulted in failure, was absence in the design of any provision to take the horizontal thrust at the tops of the tower legs. These legs were each called upon to support a load of 230,000 lbs. With an inclination one-tenth of their height there would be a horizontal force of 23,000 lbs. at the top of each post. This force pushed in the side of the tank where the tower leg was connected and destroyed its circular shape, causing the waves in the foot walk. It also caused the outward bow in the columns below the first strut. It is an astounding fact that though the builders of this tank had to use jacks between the tops of the tower legs to keep them apart during construction, they removed these jacks upon completion, leaving nothing to take this force.

The remedy in this case was to jack the posts apart at the top and insert struts inside the tank to take the thrust.

The lesson from this poor design is obvious. It would be better not to use inclined posts, but, if they must be used, a polygon of struts should be employed at their tops that is capable of taking up the horizontal forces. If the foot walk is depended upon for this purpose, it must be well stiffened and rigidly connected to the posts. Another error in this same tower was the lack of rods connecting the feet of the posts together. The same hori-

zontal force is exerted outward at the feet of these posts. This force tends to push out the tops of the stone pedestals and still further to distort the shape of the tower legs.

RECENT DISASTROUS FAILURE DUE TO BATTERED POSTS

A tank designed in the same manner as the one above described was built at Hartford, Conn. It failed and ten men were killed. The failure is described in *Engineering News-Record,* April 12, 1923. In the issue of April 19 the reports of a number of engineers who examined the wreck are published. None of these reports make any mention of the fault in design exhibited in the heavily battered posts and absence of provision to take the thrust tending to cave in the bottom, and of course none of the reports attribute the failure to this fault in design. I was prevented from making any public comment on this failure by the refusal of more than one editor of an engineering periodical to publish my letters and articles. This is a matter omitted in engineering literature, and it is to fill up this deplorable hiatus that my efforts are aimed.

EXPLANATION OF HARTFORD TANK FAILURE

The tank at Hartford was 22 feet in diameter. It was a hemispherical bottom tank. It was carried on a tower of four steel posts, very heavily battered. The posts were 23.5 ft. high, 33 ft. apart at the base and 22 ft. apart at the tank. They are thus battered about one-quarter of their height. The inward thrust at the top of each post was about 15,000 lbs. There was an 18″x¼″ plate in the footwalk, stiffened on the outside by a 3x3 angle.

Reports on this tank state that because of the removal of diagonal rods and bottom ties in the steel tower the tank and tower rotated or twisted, and failure resulted from this cause. These posts had long riveted connections to the tank which should resist any tendency to start rotation. The bases of the posts were anchored to a reinforced concrete floor. (The steel tower was built inside of a brick tower.)

There is no doubting of the fact that the removal of bracing is a serious thing, but it is more rational to attribute the failure to lack of provision against this undisputed force of 15,000 lbs., not provided for in the design, than to initial twisting of the tower. There are many, many structures standing whose columns are not as well stiffened as the columns of this tank were before the collapse.

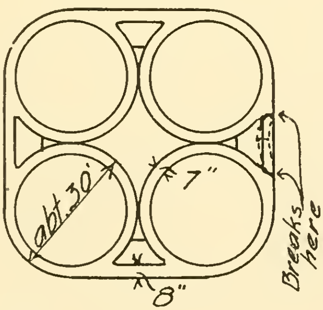
The twisting of the tank and tower is easily accounted for. It would be practically impossible for the thing to fail symmetrically. Rotation is the most natural thing to look for after one of the legs of the tower had pushed in the side of the tank. One leg would be expected to fail first, as was the case in this tank.

EXPANDING CONCRETE BURSTS TANK

A pressure tank in a mechanical filter plant is 25 ft. long and 8 ft. in diameter, with dished heads. The tank is placed with the axis horizontal. The bottom of the tank, as it lay was filled with a segment of concrete about 1½ feet deep. Shortly after this tank was put into use, being filled with water under a pressure of about 75 lbs., the head burst out, ripping for about one-third of the circumference and springing out about 5 inches at the widest. The internal pressure of 75 lbs. was not enough to overstress any part of the tank: in fact the tank had been tested to over 100 lbs. The material and workmanship were excellent. The author, who was called in consultation, concluded that the failure was due to expansion of the concrete upon being water soaked, aided by the internal pressure of the water. Concrete setting in this condition, if water were left over the surface during setting, would expand, as concrete always does when setting under water. It is also true that concrete swells slightly upon being wetted. The initial tendency to expand, augmented by the swelling and the internal water pressure, offer ample grounds for the conclusion that failure was due primarily to the solid and continuous block of concrete. The remedy is of course to make joints in the concrete or to keep it back from the ends of the tank.

SPACES BESIDE CIRCULAR BINS NOT PROPERLY DESIGNED FOR STORAGE

More than one grain bin or tank has failed where the spaces between circular bins have been used to store grain. The reason for the failure ap-



Bins about 90' high
Fig. 4 Plan of Grain Elevator at Springfield O. Showing Collapse of One Bin

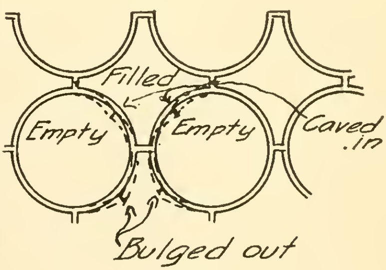


Fig. 5. Grain Elevator at Duluth Showing Collapse of Star-Shaped Space

pears to be that while the circular bins were reinforced for their stress when holding grain, the spaces between them were simply enclosed. One

of these failures is described in *Engineering News,* Nov. 3, 1910. Fig. 4 is taken from that journal. The figure shows how the failure occurred. The slab which broke out did not have any anchorage into the side of the circular bin. Even if it had had such anchorage, this construction is faulty. It is obvious that a slab having a span of 20 feet should not be anchored into the shell of a cylinder only 7 ins. thick and pull directly away from the same. If intensified economy of space is necessary, some other solution of the problem should be sought.

A nest of reinforced concrete grain tanks at Duluth failed twice from the same cause, namely filling the star-shaped space between cylinders. These two failures are described in *Engineering News,* Dec. 27, 1900, and April 30, 1903. Figure 5 shows the manner of the first failure. It is evident that the pressure of the grain in the star-shaped space did not correspond to water pressure on the quadrants of the cylinders. This pressure was greater at the middle of the quadrant, because of the greater width of grain opposite this portion. There was therefore a bending moment, and the quadrants did not act as arches against a uniform hydraulic pressure. This accounts for the caving in of these bin sides. The deflection resulting from the cave-in accounts for the other breaks.

The lesson to be drawn from these failures is that the mere fact that there is an enclosed space between these circular bins is not warrant for filling this space with grain or seed. The pressures produced by the material stored in these spaces must be provided for, and failure to make this provision may result in the wrecking of the cylindrical bins and the necessity for rebuilding the same.

ROOF OF GASOLENE TANK FAILS

A 55,000 barrel gasolene tank, 114 ft. in diameter and 30 ft. high, with a conical steel roof made of $\frac{3}{16}''$ steel plates failed in the roof in a peculiar manner. The entire tank was air tight including the roof, which was cone shaped with a rise at the center of 8 feet. There were a man hole and two gage hatches in the roof: these were supplied with gasketed covers. The only other vents were two gooseneck pipes near the top in the shell. These had been plugged for a water test. Gasolene was pumped into the tank, but no provision was allowed, by opening any vents, for expansion and contraction of the air and vapor in the tank. Gasolene vapors form at comparatively moderate temperatures, and these vapors condense when the temperature drops. The heat of the sun warmed up the contents of the tank and vaporized some of the gasolene, and the chill of the night condensed the vapors and reduced the air pressure. The result was a partial vacuum, and the steel roof caved in breaking all of the 19 posts supporting the same. The roof sheets and the riveted seams of the same were not injured. The

cone of the roof was reversed, and practically the entire frame of rafters and circular beams hung below this reversed cone. The sole means of support this frame work had was the connection of the ends of the outer row of radial rafters to the shell of the tank and the connecting bolts in the flanges at the intersections of rafters and circular channels.

It became my duty to determine the cause of this accident and to devise means of reconstructing the roof. The cause was as already stated. The lesson is obvious. A tank of this sort should have vents or breathing valves, preferably valves with checks operating by light pressure so as to prevent free circulation of gases. Also screens or baffles to keep any flame out of the tank are necessary.

The work of restoring this roof was of a very interesting nature. Also the experience in clearing the tank of gasolene vapors after it was emptied may be of value to other engineers. After the water and gasolene in the tank had been pumped out, and drained out of rivet holes opened for the purpose, a large fan was set to blow air in the lower man hole, the hatches in the top being open. This was operated for some hours. A steam boiler was secured which evaporated three barrels of water per hour. This was connected to the tank and the steam was forced in for fifteen hours, the hatches on top being covered with burlap and the lower man hole being closed. The fan was again set to blow air in the lower man hole and after about six hours the tank could be entered. The last of the water and gasolene and mud was swept and shoveled out, and the men entered to carry on the work of reconstruction.

Inside of the tank a central tower 10 ft. square of 6x6 timber posts was built to catch and support the center plate and the radial rafters attached thereto as well as to furnish better support for the roof sheet during lifting operations on the roof. Also two towers made of 4x4 posts and provided with rollers or casters were made, high enough for the men to reach the roof frame. Twenty oblong holes, 18″x24″, with rounded corners, were burned in the roof, being marked out to a pattern. These holes were midway between the posts and cut so that a sling could be put around the circular channels, two of the holes being located on each side of the center of the roof, just outside of the middle tower. Gin poles were inserted in the holes in the roof, made of two lengths of 6″ gas pipe coupled together, thus making poles about 40 feet long. Special hooks with a welded ring, made of 1½″ round steel, were hooked over the tops of the gas pipe poles, and for raising the roof chain blocks were used for all but the outer circle where steamboat ratchets were used.

It was found that the force necessary to reverse the cone in the roof was so great that it was bending the circular channels. The center plate was therefore cut loose and a seam across the roof also opened by cutting out the rivets. In two days after this the roof was lifted to place.

The parts of the posts which had supported the original roof, and which had not been bent in the failure, were used and new parts spliced thereto. Patches, cut and punched to template, were riveted on all holes made for the insertion of gin poles, the roof sheets being punched with screw punches, and the roof was restored as good as it had been before the accident. The work of emptying and cleaning out the tank and restoring the roof required in all about two weeks.

SLENDER POSTS CAUSE TANK FAILURE

In *Engineering News*, May 19, 1904, p. 475, there is a description of a tank on a tower which collapsed in ruins. The tank was 25 ft. in diameter and 20 ft. high. It was a flat bottom tank supported on beams carried by a tower 70 ft. high. The report places particular emphasis on the flat bottom plate of the tank, which was a point of weakness. The columns are said to have had a factor of safety of 1.7 to 2.5 or slightly less The failure was not such as would be accounted for by excessive bending stress in the bottom plate, for the whole tank and tower came down in a very short period of time. Furthermore, flat plates will stand very large overstress, since by sagging they immediately act in suspension. The bottom plate should, of course, have been stiffened, but the great weakness of this structure is in the tower legs, and the factor of safety was not 1.7 nor any figure approaching this.

The legs of the tower were 12″ I-beams. An I-beam does not make a good column, unless it is stiffened at short intervals in the direction normal to the web, in which direction the radius of gyration is small. But these posts had their chief bracing in the other direction, for there were latticed braces running diagonally through the tower. In the direction in which bracing of the posts was needed the most there were alleged struts made of single 2½″x2½″ angles with a ratio of slenderness about 500. These are practically of little value and the posts were consequently in effect 70 feet long without adequate bracing. The ratio of slenderness was thus very large. Even assuming this bracing to be effective the ratio of slenderness is 170 and the ultimate strength of a column of this ratio is 10,000 lbs. per sq. in. (See Steel Designing, Godfrey, p. 154.) The calculated load on some of these columns is more than 10,000 lbs. per sq. in., hence there is no factor of safety whatever. Here is an example where reliance on the Gordon formula, as commonly used, is misleading, because in the slender ratios it gives greater ultimate strength than the absolute ultimate, which is the Euler load.

CHAPTER XI

STAND-PIPES

There is a weakness in stand-pipes that has heretofore failed of recognition, though several failures have taken place. The marked similarity in these failures is evidence that they are from the same cause and is reason for revision in this standard design. I was called upon to examine one of these stand-pipes, where the weakness had shown itself by incipient failure. I found that this structure showed evidence of going the way of more than one other which have collapsed with disastrous results. The stand-pipe examined was 115 ft. high and 20 ft. in diameter, resting on a masonry base.

At about two-thirds of the height from the base a vertical crack had developed close to a riveted seam. No other manifestations of incipient failure were found, though the inside of the stand-pipe was very badly rusted and pitted, and rivet heads were in some cases almost rusted away. This was particularly true of the 8th, 9th and 10th rings from the top. (There were 23 rings.) This appeared to be the region where the top surface of the water fluctuated.

Another condition observed was pitted rust streaks vertically below each seam, across the plates not riveted at that point, also beside the calked edge in vertical joints. Still another thing observed was blisters of paint, full of water, beside calked joints.

INSIDE OF WATER TANK SHOULD BE CALKED

The tank had been calked on the outside only. This is a mistake. An oil tank may be safely calked on the outside, because oil is preservative and not corrosive; but a water tank should be calked on the inside, so that water will not enter between the sheets of metal. Paint may be relied upon to prevent rain from entering between the sheets on the outside, since the rain water is not under pressure; but the inside should receive the calking.

Rusting of this stand-pipe was not sufficient to account for the crack in the sheet, though it was at a vertical rust streak that the crack occurred; for the crack was high, where the stress was small, and other parts of the tank were very much more highly stressed. That the plate did not continue to rip proves that it was not under high stress at the time and that other than tensile stresses caused the crack.

BENDING OF SIDES CAUSES FATIGUE IN WEAK LINE

These stand-pipes are made flimsy in character. There is very little surplus metal above that required to take the tension, and they are not stiffened, except with an angle around the top. The result is that wind on the tank has the effect that it had on the old-fashioned hoop skirts, especially in the portion above the level of the water; that is, it elongates one and then the other diameter, bulging the sides in and out. This bends the sheets back and forth. This would be intensified at any weak point such as the pitted streaks referred to. It may take years of this bending to weaken a plate to the danger point. When that point is reached an unusual filling of the stand-pipe may be just enough to set off the weakness and to start failure.

Both the maximum rusting effect and the maximum hoop-skirt effect occur in a region above the middle of the height of the tank.

In 1909 a stand-pipe at White Plains, N. Y., failed. (See *Engineering News,* Oct. 28, 1909.) This stand-pipe was 20 ft. in diameter and 80 ft.

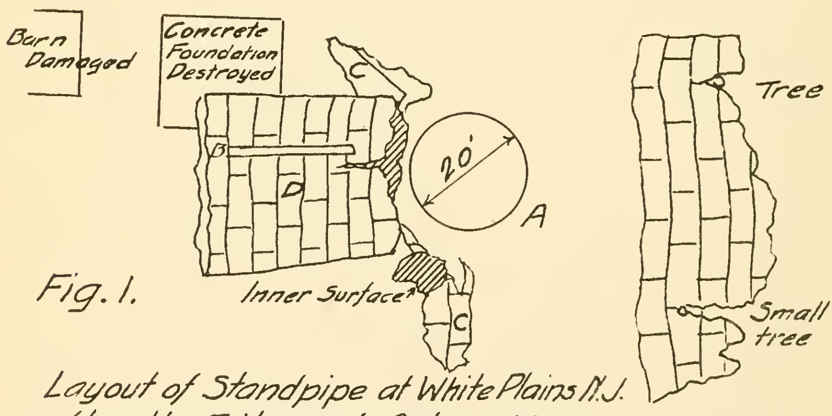


Fig. 1.

Layout of Standpipe at White Plains N.J. after the Failure of Oct 20, 1909

high. It had 20 rings in the shell. Fig. 1 (from *Engineering News*) shows the way this stand-pipe failed. The lower six rings remained standing. About the middle third ripped vertically on the left side, as placed in the figure, and a large part of this was thrown to the right by the force of the water. (See B in Fig. 1.) This allowed the upper third to topple over to the left, the side on which its vertical support first gave way. It is probable that this failure started about one-third or more of the height from the top. A vertical rip once started on one of the rusted and pitted streaks would very quickly continue down the side of the tank until a point was reached where the escaping water relieved the tension. The greater force and mo-

mentum of the water near the bottom of the break would cause that portion to be thrown farther thus accounting for the piece B landing with the outer side up.

In June, 1908, a stand-pipe at Waterloo, N. Y., failed. (See *Engineering News,* June 25, 1908.) This structure was 15 ft. in diameter and 130 ft. high, made up of 26 rings. The middle third of this stand-pipe also ripped vertically, most of the rings of the upper third held together, and the lower eleven rings remained in place. This tank was badly pitted on the inside, particularly in vertical lines below rivet heads. This was probably due to water dripping from these heads as the level of the surface of the water fell below these rivets. A weak vertical line in the shell of the stand-pipe, subjected to bending back and forth by the action of the wind, would be eventually worn out.

The remedy for this weakness is one that ought to be applied in new designs and could be applied in existing stand-pipes. It is to add extra stiffening rings to prevent the hoop-skirt action. The rings should be in fully spliced sections so that they will at the same time be capable of taking considerable hoop tension.

CHAPTER XII

SUSPENSION BRIDGES

DOUBLE CHAIN SUSPENSION BRIDGES

Until recently there was a double chain suspension bridge at Seventh Street, Pittsburgh, which was a failure in the matter of design, for the reason that the entire upper chain in the suspension spans, though it has great potential carrying capacity, was actually serving only as the upper chord of a stiffening truss. Fig. 1 shows a sketch of one span of this bridge. There are two spans similar to this, and the anchor spans. The length of span was about 325 ft. The upper and lower chains were ten feet apart. They were eye-bar chains each having the same section.

That this design is a failure is demonstrated practically by the fact that at the middle of span in each of the four trusses every one of the eye-bars of the upper chain was bowed. To be explicit: On Oct. 22, 1909, I stretched a line beside each of the eye-bars in the upper chain near the middle of span in each side of the two main spans. Every single eye-bar was found to be buckled, the amount which they bowed from a straight line being from ⅛ in. to 1 in. The averages in the four chains were .25, .32, .64, and .69 inches respectively. This condition had existed for a number of years. It may possibly have varied slightly with the temperature. There can be no doubt it would be worse under a heavy load. The bridge was practically empty when this measurement was made. The paint around the heads of these eye-bars was cracked loose, and the bars could be shaken with the hand, conditions not exhibited in the lower chain. These were further evidences that the upper chain was not carrying its load as a suspension chain.

Before this bridge was dismantled in 1923 all of the eye-bars in the top chains in every case near mid-span, were bowed with a large offset.

There is every reason to predict from theoretical and common sense considerations that these two chains could not possibly divide the tension equally, as the designer evidently meant them to do, except when they are perfectly adjusted to a given fixed condition of loading and loaded with exactly that condition. Perfect adjustment could neither be attained nor sustained. In any event a bridge should not be designed so that a slight settlement of a pier or anchorage would double the stress in a main supporting member, entirely relieving another.

It is hard to see any justification for this form of construction. Simply stated it consists of a truss span and a suspended span all in one. The combination will not annul the tendencies in either. There will always be the tendency of the truss to have compression in the upper chord and tension in the lower chord, or at least a greater tension in the lower chord than in the upper; and this difference will be greater, the greater the load carried. If the supports of such a bridge could be adjusted so as to give an initial tension in the upper chord greater than its safe working stress, while receiving only the dead load, the application of the live load might diminish this tension and equalize the stress in the two chains by putting tension

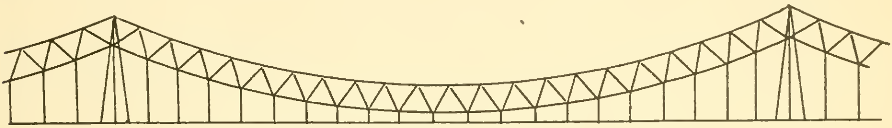
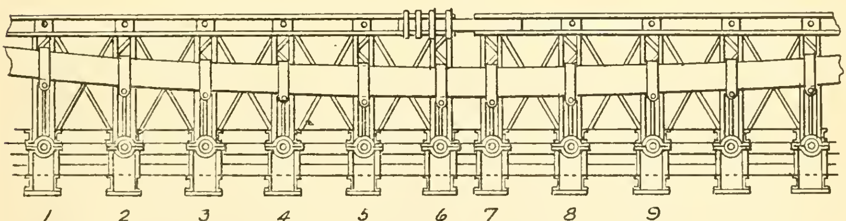


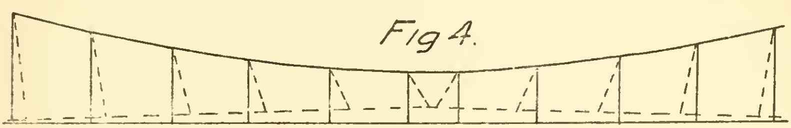
Fig. 1. Double Chain Suspension Bridge -7th St. Pittsburgh.
See Engineering News Dec. 3, 1903.
Stiffening truss improper type - Continuous at towers.

Fig. 2. Highway Suspension Bridge at Cologne.
This bridge has a proper type of stiffening truss.
It is non-continuous - Only cable is continuous at towers



Sketch Showing Broken Suspender Rods on Brooklyn Bridge Fig. 3

Fig 4.



in the lower chord. But this adjustment would be a practical impossibility and undesirable from every standpoint.

Imagine a bridge of this sort in which the two chains are under equal stress when carrying only the dead load. If any live load comes on a span,

the tendency is to deflect it. The tension in the chain will pull on the chain of the adjacent span, which must of necessity cause the towers to lean toward each other. This leaning will mean a shortening of the upper chain, or else a greater vertical deflection of the same. But the latter would not be possible, as the two chains are rigidly connected to each other. There will then be at least greater tension in the lower chain than in the upper, as the former will be stretched a greater amount.

Again, consider the effect of equal deflection of the two cables, supposing the towers to be fixed in position. Equal deflection or equal stretch of the two chains is a necessary accompaniment to equal stress. In the deflection the pins at intersections will drop practically vertical. This would mean the shortening of one set of diagonal web members and the lengthening of another set. But this is incompatible with the office (under uniform load) of these diagonals as hangers for the upper chain, as they should all be in tension. Furthermore an analysis of these web stresses would show that they unbalanced the assumed equality of stress in the two chains.

The outlines of this bridge are pleasing and the idea of the web members acting as members of a stiffening truss is sound, but instead of the lower eye-bar chain that member should be a stiff member and non-continuous at the middle of span and at the towers. The upper curve should be a tension chain capable of carrying the full load. The Grand Avenue Bridge at St. Louis, Missouri, is built in this manner. This bridge is described in *Engineering Record,* June 6 and 20, 1891. It is an eye-bar suspension bridge. The bridge illustrated in Fig. 2 is described in the *Railroad Gazette,* Nov. 20, 1903. This is a wire cable suspension bridge.

FAILURE IN BROOKLYN BRIDGE

In *Engineering News,* Aug. 1, 1901, will be found a description of a failure of suspenders in the Brooklyn Bridge. Fig. 3 is taken from that account. Nine of the suspender rods supporting the floor system broke. Some of the cable bands also broke. It is seen by the figure that there is an expansion joint in the floor system and stiffening truss at the middle of span. When expansion occurs at this joint, it will evidently swing these suspenders out of plumb. This will result in raising the elevation of the lower end of the suspenders, as indicated in Fig. 4. The shorter suspenders will have the greater rise, not only because they are shorter (as may be seen geometrically), but also because the amount of expansion and contraction accumulates toward the expansion joint. The result is that a greater proportion of the load is thrown on the end suspender because of the stiffness of the floor. This would tend to break the shortest suspender, and subsequently the next one, which would then be in a similar condition. In a very elaborate report on this failure this very vital point was overlooked. The suspenders were hinged at the ends but were not lubricated. This gave rise to heavy bend-

ing stresses and hastened the failure. The remedy is not to have the expansion joint of the stiffening truss where the short suspenders occur.

FAILURE DUE TO INADEQUATE STIFFENING TRUSS

On May 16, 1923, there was a partial failure of the suspension bridge at Steubenville, Ohio. This is a cable suspension bridge supporting a roadway and carrying a trolley line. The failure is described in *Engineering News-Record*, May 31, 1923, and photographs of the bridge which I took are published in the same journal of June 21, 1923. The structure has a channel span of about 680 feet and side spans of about 260 feet, with floor hangers spaced 16 feet. The stiffening trusses are interrupted by expansion joints at the center of the main span. These trusses are 20 feet deep at midspan and anchorages and about 25 feet at the towers.

At the time of the failure several heavy cars of cinders were on the west shore span. It was reported that this span suddenly sank six or eight feet and that the channel span rose correspondingly. After removal of the cars the structure recovered its normal shape.

Various weaknesses in the design and construction of the bridge contributed to the failure, insufficient stiffening and bracing of compression members and improper composition or design of compression members being the chief faults.

Near midspan of the west shore span the top chords of the stiffening trusses were broken, one in the seventh panel and the other in the eighth panel from the shore end. The top lateral system in the same region was badly distorted. The bottom chord buckled on a length of three panels between the break and the abutment; the bottom lateral bracing in this region had been cut during some earlier reinforcement operations.

The top chord of the stiffening truss is a T-shaped section built up of wide, thin plates and small angles. This is an improper type of compression member. Fortunately, it is not very common in America. It seems to be common in Europe, where several failures have occurred in bridges having this type of top chord, and these failures appear to have been due to the inadequacy of this make-up of compression members. American specifications for railroad bridges require that flanges on compression members be no thinner than one-twelfth of the unsupported width.

The top lateral struts of this suspension bridge are very slender T-shaped sections, while stay-struts at the crossing points of the top lateral diagonals are better designed for compression, being latticed members, but these are not part of the lateral system proper. There is practically no sway bracing, the nearest approach to it being small curved angle knees connecting the posts and the slender T-struts.

In three panels lying between the break and the abutment, new floor-

beams had been inserted, at some time in the past, between the regular floorbeams. These new beams, suspended directly from the cables, support the stringers at mid-panel. The bottom laterals, however, which interfered with the insertion of the new floorbeams, were cut apart to permit of inserting these beams, being burned off on either side of the new floorbeams. In these three panels the bottom chord buckled, and at least one of the laterals is badly distorted, showing the need of lateral bracing.

This bridge failed again in 1924. (See *Engineering News-Record*, May 22, 1924, p. 1911.) The failure was almost identical with the one about a year before, but was in the Ohio end of the bridge.

CHAPTER XIII

DRAW BRIDGES

In 1906 a disastrous wreck occurred at a draw bridge at Atlantic City, because a loose rail end, which was supposed to drop into a channel after the bridge was closed, did not drop into place and was not hammered into place by the bridge tender. In 1910 a similar accident took place. These accidents have an indirect but a vital connection with the design of the draw bridge.

RIM-BEARING VS. WEDGE-BEARING DRAW SPANS

In *Engineering News*, Dec. 20, 1906, a letter of mine discussed the relative merits of rim-bearing and wedge-bearing draw spans. The following is quoted from that letter:

"A great deal of ingenuity is expended in devising means to drive the six or more wedges upon which a wedge-bearing draw span with a center pivot is to rest when in use. If one-half of this skill were exerted to improve the good old reliable rim-bearing type of span, a bridge that is immensely safer would result.

"We have all been schooled in the awfulness of using continuous beams because of the unknown and mysterious results that might ensue if the supports did not fit the unstressed contour of the beam. We hear scarcely anything about the dire consequences of resting a girder or truss on three and one-half supports, three of which are simultaneously pushed under it. The half support is the pivot, which carries an entirely indeterminate part of the load on the center pier. There is no means of determining, even approximately, the amount of load borne by the middle wedges and the pivot. Strain sheets show a very nice distribution, allowing the wedges to take the live load on the truss and the pivot girders to carry only their own live panel load in addition to the dead weight of the bridge. This is a sort of fiat, rather than a rational assumption. A short rigid girder, with practically no deflection, cannot be made to distribute its load in any such prescribed parcels.

"A wedge-bearing pivoted span is dangerous when the wedges are not driven, as it rests on a pivot calculated to support only the dead load and a small part of the live load in the middle panel. It therefore depends solely on the operator. An old-fashioned rim-bearing span is ready for traffic the instant the tracks are in line, and any one on the street can see whether or not it is so.

"If the wedges of a wedge-bearing span are not driven home, or if they are out of adjustment, so that some come to a bearing while others do not, a condition arises that no simple assumption of separate span stresses in each arm will meet. This latter is a makeshift used by designers of these bridges to cover a fundamental weakness. It might as well be included in the 'factor of ignorance.'

"In the matter of tracks there is a great advantage in the rim-bearing spans over wedge-bearing spans. A drawbridge having a separate piece of rail, which must be lifted and lowered, adds complications that increase the danger. This was exemplified at Atlantic City. If the end floorbeam finds support on rollers at a fixed elevation, the track can be rigidly attached to both bridge and abutment, or a short extension of a few inches of rail could slide onto a rigid bracket on the abutment, thus giving a continuous track as soon as the bridge is latched.

"Much is said about the great uplift that is exerted on the far side of the drum when one arm of a rim-bearing span is loaded, and the inability to provide anchorage. The dead load of one-half of the bridge is there to act as anchorage, and calculations based on the extension of members show that the actual uplift is relatively small. (See *Engineering News,* April 16, 1903.)

"The difficulty of making a track for the drum rollers that is true and level is a drawback in the execution of plans of rim-bearing bridges. This could be greatly minimized if the track segments, instead of being bedded on the masonry and bolted to the same, were detailed to bolt to a curved girder, planed on top to a true surface. This girder need not be made of the rigidity of the drum, nor anything approaching it, but could be made of sections that will be stiff enough to retain the shape in handling. It could be placed on the pier when finishing the same, and can be readily made truly level. Blocked up in this position it could then be built into the pier with concrete or mortar, and grout where necessary. This would be a secure anchor for the track segments and the rack and a tie for the pier. The pier itself would take the load of the rollers.

"Another bugbear, namely, the hammering of the unloaded end of the span, could be readily overcome, if desirable, by building an eye-bar into the abutment and driving a pin through it and two bars in the draw, when the bridge comes into place. This would be a safeguard, the omission to operate which would have no serious consequences."

DRAW SPAN WITH POOR BOTTOM TRACK EXHIBITS FAILURE

In *Engineering News-Record,* Apr. 18, 1918, there is a description of a swing span which showed signs of failure due to wobbling of the rollers under the drum. It is a rim-bearing span weighing about 1900 tons. The

track segments are separate cast pieces bolted to the masonry. The following letter of mine on the subject of this bridge appeared in *Engineering News-Record*, May 23, 1918:

"In your issue of Apr. 18, on page 775, the failure of wheels and drum stiffeners on an important highway drawbridge near New York is described, and a remedy is proposed. I do not know anything about this bridge, but would venture a guess that the real cause of the trouble is not hinted at in the article. I think that the whole trouble is in the lower track, and until that is remedied the difficulty will continue.

"It has always been a mystery to me why, after so much care had been taken to line up the bottom of the drum and the upper track by careful planing in the shop, the bottom track should be made of a lot of short pieces of casting laid on uncertain masonry by the necessarily imperfect methods of field work. In *Engineering News* of Dec. 20, 1906, I recommended that the lower track of a rim-bearing draw span be fitted to a circular girder planed on the top, as the drum is planed on the under side, and that this circular girder be placed on the pier and carefully leveled and then concreted and grouted in. There is no doubt whatever in my mind that if this had been done in the case of this bridge there would have been no trouble.

"It is the wobbling of the wheels that has given rise to their breaking and to the bending up of the outer edges of the upper tracks and the buckling of the outer edges of the stiffeners. To my mind it is inconceivable that these effects could have happened from any other cause, and the introduction of extra wheels and the strengthening of the stiffeners can only serve to minimize the trouble: it will not eliminate it, unless the lower track is improved."

FAULTY DESIGN IN BASCULE BRIDGES

The following, quoted from my contribution to a discussion on lift bridges, published in the Proceedings of the Eng. Soc. of Western Penna., Feb., 1909, bears on defects that cause partial failure in bascule bridges:

"For most conditions where a lift bridge is appropriate, there is nothing that is better than a plain trunnion bridge. The trunnion bridge is simple of construction and easy of operation. The friction is small and the wear is almost negligible. The stresses are completely determinate, and the parts can be made in any well equipped shop.

"There is no royalty to pay on such a bridge, and hence a large item of economy is already adjusted for the purchaser. There is no royalty to receive on such a bridge, and hence there is not the incentive to capitalize or push the construction of plain trunnion bridges. This accounts for the fact that plain trunnion bridges have not received the attention that they deserve.

"In a trunnion bridge the mass is not moved along horizontally. This lessens the amount of work necessary to operate, as there is simply rotation, in a well-balanced bridge, of a balanced mass. The friction on well-oiled

trunnions is less than the rolling friction on a track that is of necessity imperfect and more or less rough. The inertia to overcome is very much less.

"Another great advantage of fixity of the dead load is in the resultant stability of the foundation. A large mass moving back and forth on a pier is a very severe test of its stability and permanence; and the requisite mass of the masonry to maintain stability, as well as the reinforcement to maintain integrity, makes a pier properly designed for a bridge whose weight is transported in operating a very expensive piece of work. A pier that is not properly proportioned and reinforced will be racked by the moving back and forth of the great weight upon it. The evidence of this fault in bridges whose mass is moved horizontally, or of the inadequacy of the piers, can be seen in some bridges already in use.

"Rocking bridges crawl along on the track. This is a fault difficult to overcome. It is evidenced in bridges of this type in Chicago, and it gives rise to trouble. Teeth or bosses on the track do not hold the bridge; they rather break out metal in the track. Still another fault in rocking bridges lies in the fact that the reaction, both dead and live, is in a single line, the line of contact between tread and track. Of course the compression of the metal makes this line a surface of some extent. It also, as evidenced in the bridges, causes the metal to flow or crush. In a rocking bridge in Chicago, the angles on the segment broke on account of excessive wear, and when the bridge was opened, these angles remained on the track, being completely severed from the segment.

"In another rocking bridge in Chicago, there is abundant evidence of excessive concentration of pressure on the segment, in sheared rivets, battered stiffeners, and crushed metal. Stiffeners at intervals do not adequately stiffen the tread of a rocker, unless the tread itself is very thick and rigid and the flange angles are very heavy, to carry the load to the web. Further, there is need of rivets enough to take the full reaction in close proximity to the application of that load. All of these features in proper proportion would demand very heavy and expensive construction. If, in addition, the girder or truss rocks on a plate or box girder, equally heavy construction would be demanded in the latter girder.

ADVANTAGES OF TRUNNION BRIDGES

"Trunnion bridges offer special advantages for double leaf spans, as the tail of the girder can be readily anchored and each leaf made a cantilever capable of independently supporting the live load. Sometimes the approach span is utilized to furnish part of the weight necessary to anchor the tail of the bascule girder. In single leaf spans the construction may be very much lighter than in double leaf spans, as the truss or girder is a simple free ended span for live loads.

"The counterweight on trunnion bridges is preferably made of regular

cast iron blocks with holes cored for bolts and the blocks securely bolted to the steel work. Sometimes concrete is used, but ordinary stone concrete would generally require too much bulk. A concrete can be made with steel punchings as the aggregate. This weighs about 300 lbs. per cu. ft., which means about 45 per cent of the volume in steel."

Some excellent examples of trunnion bridges are described in *Engineering News,* July 3, 1902; *Engineering Record,* July 12, 1902; *Engineering News,* July 14, 1904.

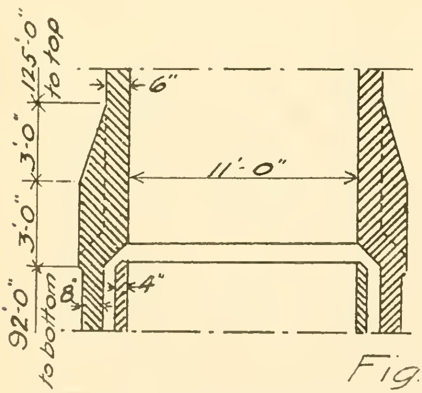
CHAPTER XIV

CHIMNEYS

A number of reinforced concrete chimneys have fallen down or have show signs of failure. Reports have been written upon these that convey but little intelligence. Several serious faults in their design and construction have not been touched upon in these reports.

REINFORCEMENT NOT PROVIDED AT CHIMNEY OFFSETS

Fig. 1 shows the section of a portion of a reinforced concrete chimney which failed. This is described in *Engineering News*, Oct. 11, 1906. The figure shows a section of the shell at the offset. As seen there was a sharp offset of 8 inches in the outside shell. The offset is here designated as sharp because of the fact that the concrete or mortar to the left of the line A B C was simply plastered on after the forms of the shell proper were removed. The chimney broke at this offset. It is characteristic of chimneys of this design to show signs of weakness or to fail at this offset. Some have bulged out and developed vertical cracks below the offset. The fault in the design is not difficult to see.



Sketch showing General Dimensions of Reinforced Concrete Chimney at Peoria, Ill., that Failed Sept. 29, 1906

Fig. 1.

It is one to which I called attention in *Concrete-Engineering* in 1907 (reprinted in my book Concrete). The weight of the upper portion of the chimney is communicated through this offset in a diagonal direction. The result is a set of horizontal components which in the upper portion of the chimney gives rise to a circular compression and in the lower

portion a circular tension. Besides this there would be a heavy shear in the offset, if, as in the case of the chimney in Fig. 1, the portion to the left of A B C were merely plastered on. The outward thrust below the offset is what causes these chimneys to bulge out and crack below the offset. The remedy for this fault is to give this offset a long sweep, in fact and not in appearance, and to use extra reinforcement at the bend of the upright rods.

The account states that 54 cu. ft. of concrete was mixed by hand at one time, and that it took two or three hours to place it, and that it was mixed very dry. This is entirely too much concrete to mix by hand at one time: it is too long a time between mixing and placing: dry concrete is utterly unsuitable for reinforced concrete.

A chimney at Louisville, Ky., of this same design, failed in 1907. See *Engineering News,* Feb. 7, 1907. This chimney broke off above the offset. It was two years old, showing that the concrete was thoroughly seasoned. The standing portion contained many cracks both vertical and horizontal. The calculated compression in this chimney was 480 lbs. per sq. in. The concrete is said to have been a 1 to 8 or 9 mixture of sand and cement. Presumably it was a dry rammed mixture. In a wet mixture this small amount of cement would be largely washed out, and it would take a long time to harden. Dry rammed concrete is standard for this kind of work—a standard only deserving of condemnation.

EXCESSIVE COMPRESSION ON UNCONFINED CONCRETE

There are several serious faults in this chimney. The offset where the diameter is reduced does not appear to have been the one that caused the collapse, though it would account for the cracks below the break. A stress of 480 lbs. in compression is too great a stress, even on good concrete, except in a hooped column or as extreme fiber stress on a beam.

The material of which this chimney was made was not good concrete. Good concrete must have the voids in the sand filled. In reinforced concrete there must be an excess of cement to cover the steel. If only sand and cement are used, there should be no more than about three parts of sand to one of cement. Dry concrete, that is, concrete of the mealy variety which is rammed into place, is entirely unsuitable for reinforced concrete. Concrete for reinforced work should be poured and not rammed.

A tile and concrete chimney which failed is described in *Engineering News,* Sept. 5, 1907. This chimney was 150 ft. high and 7 ft. 6 in. in outside diameter. It was built of tile, the hollows of which held reinforcing

rods and a substance described thus: "All concrete filling to be composed of 1 part Portland cement to 3 parts clean sharp river sand, mixed dry until of a uniform color and then moistened until the mixture has the consistency of damp clay. This mixture to be rammed into place until water appears at the top."

FAILURE TO REINFORCE BESIDE, ABOVE AND BELOW OPENINGS

One very important feature in the design of this stack is an opening in the side which was located near the bottom. This opening appears to have been 3 or 4 feet wide and about twice as high. In a plan of the chimney there is no arch shown over this opening and no reinforcement beside it. A photograph of the stack after failure shows concrete enlargements along the vertical sides of the opening but no sign of a top arch or lintel.

The chimney broke off at this opening and fell toward the side of the opening, striking a wall in its fall and pushing over a large portion of it. The chimney had just recently been completed, but the concrete was sufficiently hard near the base to hold 50 ft. of it in circular form and allow it, after forcing the wall over, to roll sidewise through a large angle. Above this 50-ft. portion the stack was shattered.

The principal fault in the design of this chimney was in failure to reinforce it beside the opening. This reinforcement requires not only thickening up of the walls beside the opening by concrete that is integral with the walls or properly bonded thereto, but also a lintel above the opening. The lintel should be horizontal and straight, that is, neither arched nor curved. An arch would not have its thrust taken care of, and a curved beam would tend to rotate. If the reinforcement beside the opening does not extend down to the foundation, there should be a similar lintel at the bottom of the opening.

IMPROPER CONCRETE

As stated before the so-called concrete used in this stack is quite unsuitable for any kind of reinforced concrete work. All concrete for reinforced concrete should be poured and puddled or churned to make it flow around the reinforcing rods and to liberate entrapped air. This is necessary in order to get protection and grip of the steel. It is doubly necessary where the concrete is poured into a narrow space as in this chimney.

In my paper, "Some Mooted Questions in Reinforced Concrete Design," read before the American Society of Civil Engineers in 1910, and

in *Engineering News,* Oct. 8, 1908, I pointed out the folly of using complex formulas for reinforced concrete chimneys, as done by standard writers, also I emphasized the error of one of these writers in recommending a unit stress of 500 lbs. in the concrete.

BRICK CHIMNEY FAILS

In *Engineering Record,* April 28, 1900, there is a description of a large brick chimney in Hamilton, Ont., which failed. The following is quoted from my letter, published in *Engineering News,* March 31, 1910. It concerns not only this chimney but also two hollow or perforated block chimneys which also failed.

"In 1900 a brick chimney in Hamilton, Ont., failed. This chimney had very heavy walls, being 5 ft. 6 ins. thick at the bottom, where failure occurred. The wall at the top was 26 ins. thick. Of course there is an appearance of safety and strength in a thick wall like this; but when it is analyzed, the very excess of thickness is seen to add to the liability to failure, for reasons that will be given presently. At the base of the chimney referred to the load per square inch on the brickwork would be about 120 lbs. The wind load on this stack would not need to be considered, even if the wall had been only a half or a third as thick. Wind played no part in the collapse, as only a breeze was blowing. Further, if the wall had been only a half or a third as thick, it would have had the same unit pressure from its own weight. It is hard to see why such thick walls were made in this chimney, which was 200 ft. high and 19 ft. 4 ins. in diameter, at the base. Unless a wall has a heavy taper, or unless its weight is needed for stability against wind, or unless its thickness is needed to reduce slenderness, excessive thickness means weakness instead of strength; for, while a thick wall being less slender will apparently be capable of carrying more unit load, there is a practical consideration that diminishes its unit carrying capacity.

"The practical consideration referred to is the manner in which a thick brick wall is laid. Everyone who has watched a brick wall being built is familiar with the way that a bricklayer will throw in dry bricks into the backing, probably putting a little ridge of mortar over them. The thicker the wall the more carelessly is the brick laid in its interior. It is the exception to see a thick wall thoroughly flushed with mortar.

"Even if a brick wall 5 ft. 6 ins. thick were flushed with mortar the lime mortar in the interior would be a long time hardening. We are told that lime mortar in the interior of very thick walls built ages ago has been found to be still soft.

"In the particular chimney referred to there were found in the interior of these fortress walls large stones in the section where the chimney

cracked and sheared off. The wall was laid in lime mortar. It is scarcely likely that these stones were flushed with mortar or that the cavities were filled. It would take bucketfuls of mortar around each stone, and mortar is more expensive than air or even stone or brick.

"It would have been immensely better if these walls had been a small fraction of the thickness they were made, and laid carefully with cement mortar or even lime mortar.

WEAKNESS OF HOLLOW BLOCKS

"The two chimneys to which you refer in your editorial (issue of Feb. 17, 1910) had one thing in common. They were made of perforated brick. Now hollow blocks of all kinds make a good talking point. They are light; they are strong; they are thoroughly and evenly burned; they are non-conductors; etc. In an office it is easy to show that the several webs and shells make a continuous compression member, for do they not butt against one another and have a neat mortar joint where they meet? These hollow and porous blocks can be very neatly laid—in the office. Out on the job it is quite different. The brick masons are not talking—at least not about the strength of a wall made of hollow blocks, nor, are they caring. They are putting up these blocks in the way their experience has taught them; they are carrying out their employers' orders not to throw away mortar by stuffing it into these useful hollow spaces. They are laying up a wall that looks nice on the outside, for there is a joint of mortar on at least two edges of the block. It is not practical to get all of these ribs just over ribs of the block below, and it is not practical to fill all of these joints with mortar without losing a lot of mortar in the holes.

"An inspector would have to be very much more vigilant on a hollow block job to insure the filling of every joint than he would on a concrete job to insure the making of good concrete, in spite of all that is said of the need of eyes on a concrete job. It is comparatively easy for an inspector to cover a large concrete job and see that good work is done in every part, but the laying of hollow blocks that take heavy pressures, as in chimneys or flat arch floors, would need an inspector for every one or two workmen.

"Furthermore, the inspector who tries to make workmen do what is 'never done' will find that he is up against it, especially if he is a young member of the 'show me' fraternity.

"It is easy to see what would happen to a wall under heavy pressure, if the blocks are bedded on two edges only, when the load is a respectable fraction of that which, in a testing machine with neat cement joints and perfectly fitting compression plates, would crush the blocks. It is

not difficult to find a plausible reason for the cracking of such a wall, or for its leaking smoke even without cracking.

"The account, in your issue of Aug. 19, 1909, of the wreck of one of these chimneys states that a large proportion of the bricks were blackened on two or more sides. This is clear evidence that the said two or more sides were never covered with mortar.

"The best way to overcome such faults due to careless workmanship is to make it impossible for the workmen to commit them, by using solid bricks or better by using concrete.

"The same fault exists in flat tile arches and slabs. Hollow tiles with the 'hollows' and the thin edges of the webs meeting cannot take much thrust, particularly as it is practically impossible to make a full mortar joint between the effective parts of the tile. They cannot be flushed, as the liquid mortar would be lost in the openings. It would be asking too much of a workman to expect him to put mortar on all of these webs. Further, the webs are often fire-cracked and curled.

"I have long been of the opinion that a large part of the strength of the ordinary tile arch lies in the wooden sleepers and the concrete filling. In the old style of arch having 5-ft. or 6-ft. spans this was not a matter of so much consequence, as the thrusts were not large, and the sleepers would carry a large load because of the short span. But I know of one bold piece of construction in a large building where, on spans of 16 ft. and over, there are slabs consisting of a 1-in. layer of cement mortar in which steel is embedded and a layer of soft hollow tile 5½ ins. deep with ¾-in. shells and webs. The floor is laid on this. Is it any wonder that reinforced concrete builders are using higher and higher units and are being shut out of competition, when such monstrosities as this pass for engineering, and are approved by city building bureaus?"

As a further light on the first part of the last paragraph it is pertinent to note that reports stated that in the Baltimore fire tile arches that were not filled up with concrete to the floor covering failed.

CHAPTER XV

RETAINING WALLS

Failures of retaining walls are numerous. They are, however, usually gradual in character and not disastrous in their consequences. Generally a sinking of the foundation or heaving due to frost are exhibited.

Some of the remedies for sinking of walls are: lightening of weight, so as to reduce the pressure; the use of a row of piles near the front edge; surface drainage in the neighborhood of the wall, so as to avoid softening of the ground by water; monolithic construction, so as to avoid unequal settlement. Some of the remedies that obviate and overcome the effect of frost are: thorough drainage of the backing; reserve strength near the top of the wall, where the effect of frost is a maximum; chamfering of the inner edge of the top of the wall, so that expanding ice at the ground level will not have a vertical surface against which to exert its thrust.

Following is the substance of an article on The Design of Retaining Walls by the author, published in *Engineering-Contracting,* Dec. 21, 1910.

"Retaining walls, like dams, were among the most ancient of engineering necessities. Like dams they are among the most recent to be accorded critical attention in the matter of their design. The design of dams is in such a primitive state that in all the works on dams, so far as the writer can discover, there is not one word concerning the upward pressure of water that may, and as a rule does, work its way into horizontal joints and under the base of a dam—the force that has been the cause of practically all the great failures of masonry dams. The great Austin dam failure was discovered to be due to this eight years after the failure. Colleges are beginning to mention it in their courses after agitation by the writer begun in 1904.

"The destructive force that is almost neglected in the case of retaining walls is the effect of frost. Much has been done in the way of working out elaborate formulas for the supposed pressure of earth against the back of a retaining wall. The exact direction and point of application of the forces acting on the back of a wall can be very nicely determined—theoretically. When the real wall is constructed, sometimes the earth will stand vertically with little or no shoring, while the masonry is being put up, this in spite of possible thousands of tons of horizontal force, which (on paper) is being exerted.

"Of course there is or may be horizontal force exerted against the back of a retaining wall. The writer would not belittle calculations, *per se,* of this force to arrive at a basis for proportioning the wall. But when the theory of pressures is founded wholly on assumptions which may at times be erroneous to the extent of the infinite ratio of a finite quantity to zero, the case is one where simplicity is more in keeping with facts than elaborate formulas.

"Before the real nature of the planets was discovered, supposed scientists worked out intricate rules to describe the paths of the planets among the stars. There is a great deal of mathematical junk in engineering books that is of no more use to anyone than those rules would be to a modern astronomer.

"When men want to know at what slope earth may safely lie and not be beaten down by storms and the changes of temperature, they do not take a lump of mud and let it slide down a mud incline to get the coefficient of friction and from this find the angle of repose. This might be done in such regular and uniform materials as dry sand or grain. But vastly more reliable data are obtained by observation on embankments that have stood the weather or have perhaps been subjected to the partial leveling that nature demands before she will permit them to remain stable and undisturbed.

"In the simple matter of the brick walls and windows of a building, if their capability to resist a wind pressure of 40 or 50 lbs. per sq. ft. were a test of their ability to stand, but few would remain standing. Here, too, experience has been the prime factor in determining the proportions.

"Many structures admit of exact calculations, and exact theories for their stresses are in place, but retaining walls are not among these. Safe proportions for a retaining wall may be arrived at in a manner somewhat similar to the derivation of the rule for the slope of embankments; that is, by observing the proportions of walls that have remained stable. Empirical rules based on such observations point to the fact that if a solid masonry wall has a height about three times as great as its thickness, it will retain ordinary earth with complete stability. The design of a retaining wall would then seem to be a simple matter for such cases, merely the making of a wall of a thickness one-third of its height. But there are other features that must be taken care of, and these are of equal importance with the thickness of the wall.

"In order to arrive at a basis for determining the stability of a retaining wall of other than rectangular shape and avoiding the excessive theoretical calculations so often made use of, it will be assumed that a masonry wall of rectangular cross section and having a height three

times its thickness is stable against the pressure of ordinary earth or fill. A fluid pressure will be assumed on the back of the wall, and friction of the fill against the back of the wall will be ignored. For stability the resultant pressure on the base must fall within the middle third. If it falls at B (Fig. 1), the edge of this middle third, there will be no uplift on the base. The shaded triangle H J K will represent the pressure on the soil. Moments around B should then balance; that is, the moment of the weight of the wall will balance the moment of the horizontal force of the backing. For a foot of length of wall, if $W =$ the weight per cubic foot of the masonry and w the equivalent fluid weight per cubic foot, we have

$$W\,h\,b \times b/6 = w\,h \times h/2 \times h/3$$

$$\text{or if } h = 3\,b,\ W = 9\,w$$

"If then the weight of masonry be 150 lbs. per cubic foot and the weight of earth fill be 100 lbs. per cubic foot, the equivalent fluid weight is 16⅔ lbs. per cubic foot, or one-sixth of the weight of the fill. It is very much simpler to consider the pressure of the fill as a fluid pressure of say one-sixth or one-fifth of the weight of fill per cubic foot than to use the intricate formulas for earth pressure which fail so conspicuously to justify their existence.

"A retaining wall of any considerable size is a heavy structure, and heavy structures demand special provision against settlement. Settlement in a retaining wall is tantamount to failure, because it is invariably unequal settlement, disturbing the perpendicularity of the wall, destroying its alignment and giving rise to cracks. In other structures, such as buildings, settlement may take place without serious consequences, if that settlement be small, provided the design has been made with a view of making the settlement uniform. This latter can readily be done by the simple expedient of making the areas of the footings proportional to the load. In a retaining wall the greatest pressure will be under the outer edge of the wall; settlement here means that the wall will lean outward. It is also true that the smallest resistance of the soil against pressure is at this same outer edge, since soil will resist very much less pressure near its surface than at a point some distance below the surface. Furthermore the conditions in a retaining wall are unfavorable in the matter of the softening of the ground by the action of the rain at the very place where such softening has the most harmful effect. Ground water at the base of a retaining wall will soften the soil and make it more yielding. Sometimes in railroad work there is even a gutter at the base of a retaining wall. These conditions do not exist in a building where the cellar is kept dry, and the

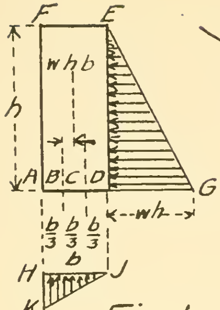


Fig. 1.

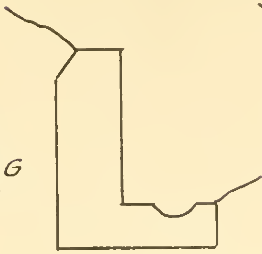


Fig. 2

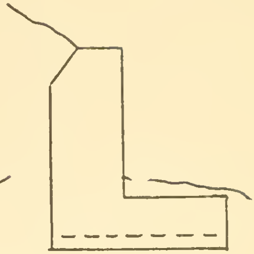


Fig. 3.

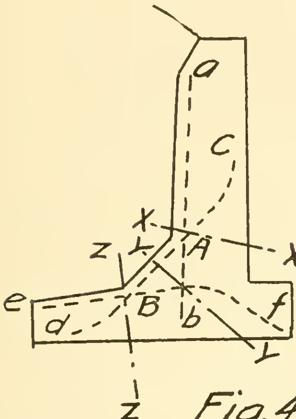


Fig. 4.

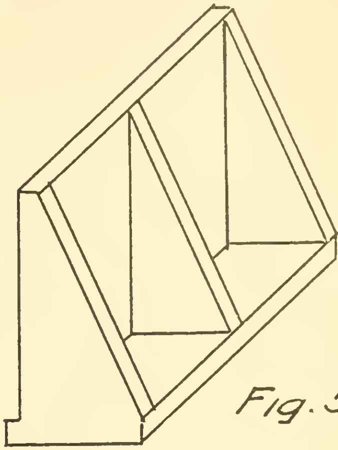


Fig. 5.

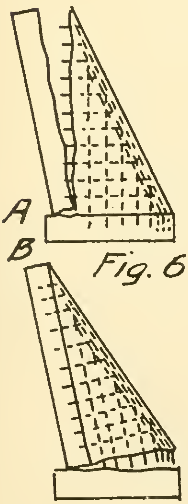


Fig. 6

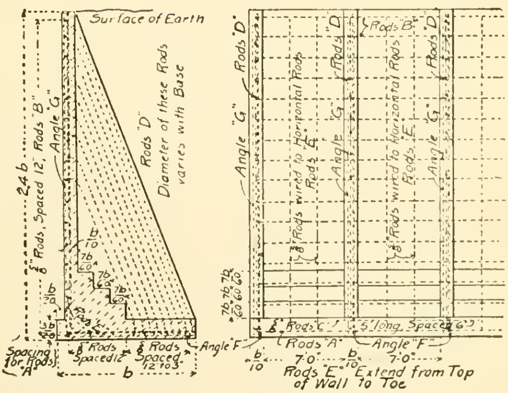


Fig. 7.

surface of the soil that is nearest to the level of the footings of walls does not undergo this softening action.

"One way that would suggest itself to overcome the difficulty would be to build the retaining wall deeper. This not only adds largely to the expense but increases the soil pressure, thus defeating to some extent its own end. Another way is to drive piles under the wall near the outer edge of the same, so as to increase the bearing power of the soil at this critical locality. This would no doubt be an excellent and economical method of insuring the stability of a retaining wall in many cases.

"Still another method is to use a wide slab for a footing under the wall, as indicated in Figs. 2 and 3. In stone work flags would have to be used. In mass concrete of course a concrete slab could be used. A little steel reinforcement would add greatly to the efficiency of the slab. In railroad work the same slab might be made to act as a gutter, carrying the water away that might otherwise act to soften the soil at the base of the wall.

"It is seen from Figs. 2 and 3 that by anchoring the wall into the slab at the back the stability against overturning would be very greatly increased. This cannot well be done in ordinary masonry, but in concrete it could very readily be accomplished.

"It is further seen that if the slab projected under the soil at the back of the wall, still more stability would result, by reason of the fact that the slab would be loaded with a large weight of superimposed earth. A T or L shape is then a logical shape for a retaining wall. Such shapes are not possible in ordinary masonry; that is, they would not have the required strength. In reinforced concrete, however, they are both practicable and economical. The projection at the front of the wall does not need to be very large, except where the soil is particularly soft. At the back the projection should be a large fraction of the height in order to take hold of sufficient earth to give the required stability. The wall will then be L-shaped in cross section.

"A simple L shape of uniform cross section in a retaining wall would mean that the base slab and the vertical wall would have to act as cantilevers. Cantilever slabs are not economical, and they are difficult to reinforce. Two cantilever slabs meeting at right angles would be specially troublesome to reinforce because of the difficulty of carrying stresses around the corner.

"Fig. 4 shows a suggested form for an L-shaped wall. Three series of reinforcing rods are used. Rods *a b* would reinforce the cantilever whose critical section is *X X*. Rods *e f* would reinforce the cantilever whose critical section is *Z Z*. Rods *c d* are needed to reinforce the corner. The section *Y Y* could be used as the critical section. If a

single curved rod were used in place of the three from *a* around to *e*, the tendency, when this is under tension, would be for the rod to pull out and break off the overlying concrete. Each rod can in this arrangement run beyond the points *A* and *B* far enough to get the full anchorage value, say 50 diameters. Rods *c f* can further be curved down, as indicated, to reinforce the projection at the heel of the wall.

"In order to make the two slabs of an L-shaped retaining wall act as simple beams and not as cantilevers these slabs may be joined by ribs or counterforts at intervals, in which rods are embedded. An oblique projection of such a wall is shown in Fig. 5, viewed from the rear.

"It is an absurdly simple proposition that the rods in the counterfort or rib act merely to tie together the horizontal slab and the vertical slab. The vertical slab is subject to horizontal pressure from the earth back of it. The rods in the ribs resist that pressure, and in order to have something against which to pull they must be anchored down into the horizontal slab, which is able to resist the pull by reason of the weight of earth upon it. In spite of the simplicity of this proposition this three-cornered counterfort is analyzed in books as a beam. Besides this, in nearly all the walls built on this plan rods are thrown in in a most senseless and wasteful manner. In one example, of a wall 45 ft. high, there are nearly 100 horizontal rods varying from 3/8 to 1 inch square, besides 22 rods 1 1/4 inches square placed diagonally close to the back, all of this in a single counterfort. In this structure there are tons of steel positively wasted. In other examples there are meshes of horizontal and vertical and diagonal rods that cannot possibly be good for more than a small fraction of their tensile strength.

"Fig. 6 shows the two ways in which one of these retaining walls could fail. The real strength of all this mass of steel rods is merely the little anchorage value of the short ends that are embedded in the vertical and the horizontal slabs. It is a cardinal principle of good design in reinforced concrete that a short end of a rod is entirely inadequate to anchor it for its full value.

"Another glaring fault in the common method of design, illustrated in Fig. 6, lies in the useless intermeshing of the rods. A single curved rod running from the horizontal to the vertical slab and anchored in each would perform all and more than all the duty that a horizontal and a vertical rod, as shown in Fig. 6, can perform. The long rods lying near vertical and horizontal slabs are absolutely useless for the greater part of their length. Single short rods from one slab around to the other, with end anchorage in each, would be far more effectual than the pairs of long rods.

"Another serious fault in this common design lies in the difficulties and uncertainties in the matter of execution. A mesh of rods in any event is a serious menace to the proper placing of concrete. A little misplacement of a rod whose short end is depended upon for anchorage could result in the rod being totally useless. Rods could very readily be omitted, even under the eye of a watcher, where such a large number is called for. It would be extremely difficult to hold one hundred loose rods in place during the pouring of concrete and to be sure that none of them has been dislodged in the operation.

"The writer has devised a scheme for reinforcing a retaining wall such as that shown in Fig. 5 that obviates all of these difficulties. The design is shown in Fig. 7. It is to be noted that the rods in the ribs, instead of being indiscriminately placed in horizontal and vertical positions, are all placed diagonally. Each rod has an easy curve near the end and is anchored for its full value in both the horizontal and the vertical slab. The office of the rib is here recognized as being merely to protect these rods from corrosion. It is not a beam in any sense and should not be filled with a useless lot of shear rods.

"The anchorage for the rods in the design shown is an angle through which the rods pass. The rods have a nut on each side of the metal to hold them in place and in bearing against the metal. The same angle has holes in the other flange through which rods pass that are used to reinforce the slabs. For a wall of this sort to fail this anchoring angle would have to be torn out, and it would pull with it the slab reinforcement. Besides being designed for the actual stresses in the wall and amply capable of resisting them, this reinforcement has many practical advantages.

"There is no mesh of rods in the rib to interfere with the flow of concrete. The rods are parallel and almost in the direction of the flow of concrete; they are further in only one plane.

"The entire harp-like reinforcement of a rib can be put together on the ground and raised to place as one piece. A few braces will hold the whole in place, and interference with placing the concrete is for this reason also reduced to a minimum

"The small number of rods makes omission of any of them noticeable. Furthermore such omission would show up in vacant holes in the angles and could readily be detected.

"It is almost impossible for any of the rods to be displaced during the pouring of the concrete.

"The writer has devised a standard retaining wall with proportions and reinforcement as shown in Fig. 7. The several features will be taken up in order. It will be noted that the height of the wall is 2.4 times the base. It was found by trial using three walls having bases 5, 10, and

15 ft. respectively, and heights 12, 24 and 36 ft. respectively, earth at 100 lbs. per cubic foot and concrete at 150 lbs., that walls of the dimensions shown in Fig. 7 would be stable against a horizontal fluid pressure of about 18 lbs. The stability of these walls would then be equivalent from this standpoint to that of walls of solid concrete having a height three times their thickness. Furthermore the stability of the reinforced concrete retaining wall is based on the assumption that there is a plane of cleavage vertically over the back edge of the slab, as only the weight of earth directly above the slab is considered. If the wall should turn over, it would have to lift more than this portion of the backing, so that the reinforced wall has this extra element of stability.

"The volumes per bay for the three walls referred to (12, 24, and 36 ft. in height) were found to be 96.6, 452.8, and 1,168.5 cu. ft. respectively. Solid masonry walls of the same height and having bases one-third the height would contain 3.73, 3.39, and 3.14 times as much in volume of masonry, respectively, as the reinforced concrete walls.

"The centers of gravity of these walls were found to be 1.43 ft., 3 ft., and 4.66 ft., respectively, from the heel of wall. The respective volumes of earth over the slab are 332.3, 1,377.2, and 3,206.3 cu. ft. The centers of gravity of these volumes are located at 2.96 ft., 5.97 ft., and 9.01 ft. respectively from the heel of wall.

"One peculiarity respecting these three walls is that the resultant center of gravity of earth and concrete is in each case almost exactly in the center of the slab. This makes it proper to assume that the pressure due to the weight is uniformly distributed over the base, as shown at *a*, Fig. 7, as the stiffness of the bottom slab is sufficient to give this distribution. The reaction of the earth under the slab will be uniformly varying from zero intensity at the toe of the wall to double the intensity of the uniform load at the heel of the wall, as at *b*. The difference between these sets of forces, or the forces shown at *c* must be resisted by internal stresses in the concrete. On the left half of the bottom slab the forces are seen to be upward. If the construction were a reinforced slab, it would require the principal reinforcement in the upper part. For simplicity of construction it is desirable to avoid this. The close proximity of the vertical wall makes it possible to throw this force directly into that wall by means of the steps at the junction of wall and slab. These steps could of course be replaced by a chamfer or slope, if the latter were found to be simpler of construction.

"The work of the steps at the junction of vertical wall and slab is to resist an upward pressure varying from zero at the foot of the steps to a maximum under the vertical wall. Reinforcement is not

needed in these steps, because of their mass and because they are supported on three sides.

"The projection at the heel of wall does not need reinforcement because its amount is only one-third of the depth. If a large projection were needed here on account of soft soil in the foundation, reinforcement should be used.

"The rods in the ribs should be bent with easy curves and not with sharp bends. Sharp bends are totally out of place in rods reinforcing concrete. The radius of the curve of bend should be about 20 times the diameter of the rod.

"A retaining wall built on this or any other plan should have the backing well drained. This plan admits of ample provision for drainage by means of weep holes through the slabs In order that these do not fill with mud, some loose stone should be laid around them.

"In a wall built after this plan at Melwood Avenue, Pittsburgh, the bottom slab was inclined. This made a pocket into which drainage could be carried. The maximum height is 64 ft. The anchoring pieces are 5/8-inch plates instead of angles. Plates do not afford a ledge or shelf for the concrete slab to rest upon, but in this case this was offset by the fact that the reinforcing rods for the slabs were curved in an arc. However, these rods, where they entered the 5/8-inch plate, were bent normal to the plate. This makes anchoring value of the plate to depend largely upon shear in these rods. Of course adhesion to the side of the 5/8-inch plate will help to anchor the plate, but adhesion of concrete to a flat surface is not of very great value, because the element of grip is lacking.

"The rods in these ribs vary from 1½ in. to 1⅝ in. They have forked loops at the ends and turnbuckles for adjustment. They are connected to the anchor plates by means of cotter pins. The front wall is 18 ins. thick, the bottom slab is 2 ft. thick, and the ribs are one foot thick. The spacing of ribs is 10 ft. and the maximum width of bottom slab is 29 ft. 6 ins. At the highest portion of the wall there is about 2,400 cu. ft. of concrete per bay. A solid wall 64 ft. high and having a base one-third of the height would have nearly six times this amount of concrete in 10 ft. of its length. The economy of this form of retaining wall is therefore manifest."

Following the article above quoted there were some letters published in *Engineering-Contracting* criticising my statements. One critic aimed to show that a cantilever retaining wall is more economical. I replied by showing by analysis some serious faults with a common form of cantilever retaining wall, the one shown in Fig. 8.

There is no doubt whatever that the counterfort retaining wall de-

scribed in my paper has a large amount of reserve strength not present in the cantilever design, and that the counterfort wall would stand very much more load under test.

The wall shown in Fig. 8 has several objectionable features. The rods A G E and H G B have but little anchorage beyond the point of their maximum stress, namely at G. It was contended that these rods are embedded in concrete in compression and hence the anchoring value was enhanced. This is an error, as the concrete in the neighborhood of G is in tension. This is the point of greatest intensity of tension in the whole wall. For more than half of their embedment these rods are in concrete that is subject to tension. In any event the idea that concrete in compression will hold a rod with greater force than that which is subject to no stress is far-fetched and purely theoretical. Concrete is not like rubber, that it can be squeezed down to take a better hold on embedded steel.

The stresses in the sharp re-entrant angle of this cantilever wall are uncertain. Sharp corners generally afford a starting point for failures, especially when there is tension existing at these corners. It is certain that a steel beam bent at right angles would not stand anything like its full strength at the bend. If steel would not stand such a test, why invest reinforced concrete with such theoretical strength? It is for this reason that I would have a bevel joining the two cantilevers and an extra set of rods, as indicated in Fig. 4.

But there is a serious weakness in the design shown in Fig. 8, that analysts have so far failed to point out. When compressive or tensile stresses must take a sharp turn, some provision must be made to take up the resultant stress dividing the angle at which the two stresses meet. The compressive stresses of the two cantilevers meet at the outer corner of this L, and there is no provision to take up this component, excepting tensile and shearing strength of the concrete. The sketch shows how and why it could easily fail. The failure would leave all the steel reinforcement intact and would be purely by tension and shear in the concrete. The arrows show the compressive stresses that are not provided for. They act against the concrete block ABCF. In addition to these stresses there is tension on the lower half of the surface A B due to the upward load on the cantilever CB. This would help to break off the block ABCF. Besides this there is a shear on the surface A B amounting to nearly the whole upward force against the base of the retaining wall. This little block of concrete is working against great odds and has large responsibility. It is not much wonder that reinforced concrete has received so many black eyes in horrible failures, when so little attention is given to correct analysis in its design. In Fig. 4 this block

of concrete is purposely left out in figuring the main cantilevers, though it is amply reinforced for its own stress. Also the two cantilevers are joined by a bevel so as to maintain the depth at the corner.

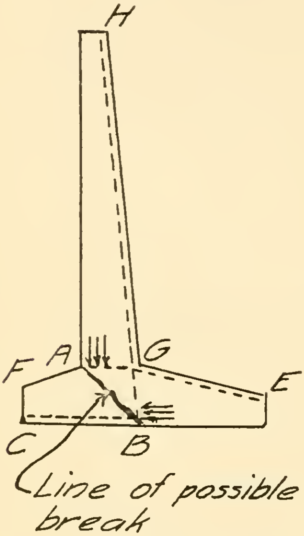


Fig. 8.

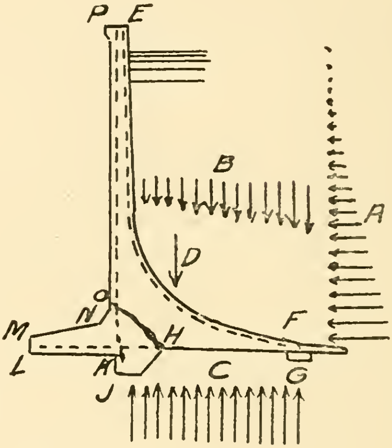


Fig. 9

Fig. 9 shows a variation of this cantilever retaining wall. This is the section of the wall of a large reservoir in San Luis Obispo, California, which failed. (See *Engineering News*, March 16, 1911.) This sketch was published in connection with a letter of mine in *Engineering News*, April 20, 1911, which explained the probable cause of the failure and pointed out the serious fault of the design. The following is quoted from that letter:

"The side of this tank was designed as a retaining wall and not designed for hoop tension. The small amount of circular reinforcement is ample demonstration of this. This reinforcement is small enough to be negligible. The forces on this retaining wall are easily determined and definite. Liquid pressure admits of exact calculation.

"It could scarcely be hoped that the thin floor of this tank could be made proof against leaks. This would be practically impossible. This means that oil was under the floor as well as over it; and the oil under the floor would exert practically the same pressure as that over it, because its escape is purposely made difficult in the design. The figure shows in a general way the forces on the wall. A represents the horizontal force of the oil against the side. B represents the pressure downwards on the horizontal portion of the "L." C represents the pressure upwards under this "L." D represents the weight of the retaining wall.

Since B and C about balance, with a possible upward resultant, it is easy to see that A and D combined will give a resultant falling outside of the base K G. This throws very heavy stress on the little cantilever L K. This cantilever is very poorly reinforced. Along the line O H there is no reinforcement for the tension of this cantilever. Besides the tension of the surface O H there is a heavy shear from the upward reaction of the soil on the surface L H. The tendency of the cantilevers O P and O M is to come together like a pair of shears hinged at O, and all there is to resist this (ignoring the hoop tension of the entire wall) is the small tensile strength of the concrete on the surface O H.

"It is very probable that the wall cracked and broke along the line O H, just outside of the rods L H. This would not disturb any of the main steel reinforcement except to bend over the vertical rods at O.

"The only remarkable thing about the whole occurrence is the fact that so much oil could be held in the tank even for a little while, but this was probably because it took some time for the ground under the tank to become soaked. However, the tensile strength of the concrete may not have been sufficient to sustain the pressure, even assuming no leaks in the bottom."

While the wall shown in Fig. 9 is an improvement over Fig. 8 by reason of the curve on the inner angle of the L, both are faulty in the matter of reinforcement. This retaining wall with the sharp inner angle is given as standard by several well known authorities. One serious feature is that the wall is not thick enough to afford adequate anchorage for the reinforcing rods of the cantilevers

The sharp angle in the L-shaped cantilever wall is not analogous to that in the counterfort wall, since in the latter the slabs are not cantilevers which tend to open up in the sharp corners. In the counterfort wall the slabs are supported by the counterforts, just as a slab might be hung, through the medium of proper anchorage, at the bottom of beams.

TWO PITTSBURGH FAILURES

Two failures in retaining walls occurred in Pittsburgh, Pa., in recent years. Great damage was done by the first of these failures, which is described in *Engineering News-Record,* Nov. 25 and Dec. 2, 1920.

The other failure was exhibited by cracking of a concrete wall due to freezing of water behind it. The first failure was in the nature of an earth slide which carried the retaining wall with it. A deep gulley near 17th Street had been filled many years before the accident happened and beside the P. R. R. tracks a retaining wall had been built. The gulley was in the side of a steep hill. Water in the soil of this fill softened and lubricated it, and the burden of its inclined pressure was

too great for any ordinary retaining wall. The whole mass of earth fill in this gulley began to flow and not only did it push out the retaining wall at the foot of the slope, but it also lifted the railroad tracks. No ordinary retaining wall would have been stable against this avalanche of earth. It would have required a structure of great stability, unless a system of drainage could have been installed that would have drained the soil down to the original surface of the gulley. The rock on the hillside is shale, and only some treatment of the problem that takes into account the slippery and fragile nature of shale would have solved this question of stability. A revetment wall, with a substantial foundation, would doubtless have remained stable.

The other Pittsburgh failure was that of a retaining wall not far from the location of the failure just described. This wall is on the upper side of the boulevard above the Union Station. A vertical concrete retaining wall had been built against the crumbling rock on the hill side, and the drainage was inadequate. Frost pushed the wall out and broke it. Just below this by the P. R. R. tracks is a revetment wall, that is, an inclined stone wall built against the crumbling rock. This wall has stood for many years and is an example of the proper solution of this problem, though revetment walls are said by some authorities to be a discarded type of construction.

A cellular retaining wall consisting of two walls three feet apart, filled with clay or earth in the three foot space, failed. This wall was at St. Louis. (See *Engineering News,* Sept. 26, 1912.) The two thin walls were supposed to be tied together by cross walls at 20 ft. intervals. Earth confined in a narrow space will not exert much pressure against the walls so long as it is dry, but if water is allowed to enter, the pressure will approach hydraulic pressure. The pressure that wrecked this wall was no doubt caused by water that accumulated in the box between these walls.

CHAPTER XVI

VENEER IN WALLS AND COLUMNS

Many instances have come to my notice where veneer on walls or columns has failed.

In one case a brick wall had a cast stone veneer 4 in. thick. This cracked and spalled in a way that showed that it was heavily loaded. In another case a concrete column had a cast stone veneer, and in another a rubble stone wall had a cut stone veneer. In another case a brick wall had a terra cotta veneer.

The cause of these failures is a simple one to explain. The main wall or column, which is or should be designed to carry the full load shrinks in setting. This throws the load on the veneer, and failure results. The reason the veneer and the backing do not shrink in the same amounts is because the veneer is in high blocks with thin mortar joints, whereas the brick or rubble wall has many more joints and much more mortar in its make-up. Concrete and mortar both shrink in setting.

There are several ways of overcoming the difficulty. A course of veneer blocks may be left out (wooden blocks being temporarily used) and subsequently inserted when the wall has set. Or the mortar may be all raked out of some of the joints and these pointed after the wall has set.

HARD BRICK

Hard, glassy brick used for veneer sometimes cracks from the effect of temperature especially when laid in strong cement mortar. Sometimes the use of lime mortar is a remedy for this trouble. This hard brick work is very apt to disintegrate in the mortar joints by reason of the fact that any moisture in the wall is absorbed by the mortar and remains in the same, because the brick will not absorb any part of it. The high saturation of the mortar joints, when freezing occurs, causes great expansion and the breaking of the bond. It is very important that water be prevented from penetrating into a wall built of this type of brick.

EFFLORESCENCE

Window sills and copings should be designed so as to turn water perfectly, whether the wall is of non-absorbing brick or of porous brick. Efflorescence in walls, that is, the white discoloring so often seen on brick walls, is largely

the result of water entering in the top of a wall and issuing on the surface, where dissolved salts out of the brick and mortar are deposited as the water evaporates.

COMBINATION WALLS

Failures due to combination walls are not infrequent, and sometimes they are very disastrous. The combinations are sometimes tile and brick: sometimes they are common brick and face brick.

A wall composed of a yielding shell and a rigid shell is not a proper combination for the support of a load, even though these shells be bonded together. For the same reason that veneer on a shrinking wall or pier frequently fails because the load is thrown on that veneer, a combination wall composed of materials having a different resistance is apt to fail.

In a building having facing of pressed brick and backing of common brick a very disastrous failure occurred. Reinforced concrete girders and slabs carried on walls and piers of this character caused the supports to bulge and collapse, and a large part of the construction came down in ruins. Walls that did not collapse were very badly bulged, due to the load of the floor coming on the pressed brick facing. The reason that the load was thrown on the facing is because the mortar joints of this part of the wall were thin, while those of the backing were thick. The greater shrinkage of the backing relieved this part of the wall of load and the more rigid shell of the facing was compelled to carry the load. (This was a building which I was called in to examine at the time of the accident.)

A large school building at Binghamton, N. Y., collapsed. (See *Engineering News-Record*, Sept. 27, 1923, p. 528.) The exterior walls were of interlocking tile with a brick facing. The interior walls were of tile. These inner walls had many openings—some very wide openings spanned by large reinforced concrete girders. It may be that these girders were inadequately supported on tile. The interior walls were probably weak. The great weakness of this building was doubtless the exterior combination walls.

KNICKERBOCKER THEATER

The Knickerbocker Theater, the roof of which collapsed in January, 1922, and in which failure about one hundred people were killed, had combination walls, and this probably had much to do with the cause and the manner of the failure. The walls were a combination of tile placed with the cells vertical and a facing of brick.

A tile wall cannot be commercially built strong when the cells are placed vertical. The reason is that there is no surface to receive the mortar for a horizontal joint. No tile layer is going to putter around laying mortar on the thin ribs of a tile. In any event these ribs may not "register" in two tiles one

above the other. The workman will put on just enough mortar to steady the tile and furnish a joint on the face of the wall. If a close steel mesh were laid in each horizontal joint, a good joint could be effected, but this is only practicable in a laboratory. Single tiles are stronger when tested in a laboratory with the cells vertical than with the cells horizontal, but this fact has an academic interest only when walls cannot be built with good joints with the cells of the tiles vertical.

There were other faults in the design and construction of the Knickerbocker Theater. In fact every part of the construction was weak, including the roof slabs, the roof beams, the roof trusses, the columns and the walls. But it was not general weakness that caused the failure. As is generally the case, a fundamental principle of stability or equilibrium was violated. At least two principles of stability were violated, and the probabilities are that these contributed jointly to the failure. Little emphasis has been placed on these two points of weakness, but much emphasis has been placed on the general weakness of the structure. No lesson of value is to be learned by emphasizing the folly of general weakness in a design, if that emphasis obscures some subtle fault. Every conscientious engineer avoids such things; a wreck is not needed to point the lesson. But there are certain principles of stability which are partially or totally obscured in works on engineering, so the poor designer, who may not have dug out these principles for himself, is not aware of the unstable equilibrium of his structure. It is these principles that ought to be blazoned on the housetops whenever a wreck demonstrates their importance.

The other great weakness of the design (in addition to the combination walls) is the support of main roof truss T 11. Fig. 1 illustrates the roof plan of this building. Truss T 11, on which the great part of the roof was supported, rested on a combination wall at an angle of nearly 45 degrees. It was not anchored to the wall, and by the process of expansion and contraction it gradually worked off the wall. The seat in the wall was a steel beam. Probably this beam was low on the inner edge of the wall, due to the yielding of the tile shell of the wall, and this slope aided the truss in sliding off the support as it did. In any event the failure can be explained in this way, and this explanation fits every feature of the wreck. In Fig. 2, suppose ABCD is the original position of the truss on the wall. After the first contraction points B and C move to B′ and C′ respectively. In expanding, instead of forcing these points back to B and C, the truss chord will slide along the edge of the I-beam, and B and C will be in the positions B″ and C″. The perpendicularity of the truss is thus destroyed, and also the wall is impaired by the force pushing it outward. The wall was actually pushed out about 5 in. at the top, when the truss gave its final kick and slid off the wall.

The progressive failure of this roof truss by gradually slipping off the

wall would not show up in the ceiling, as one engineer claims, because of the fact that the end panel of the truss was free, the ceiling beams being supported a panel length away from the end of the truss.

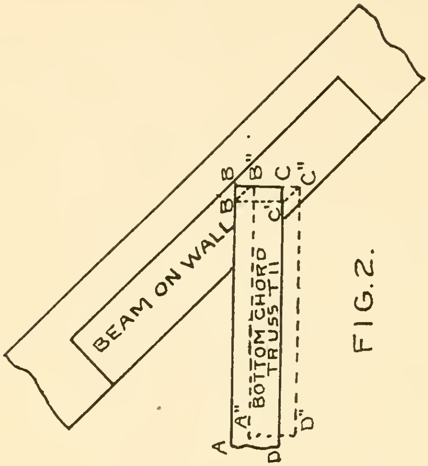


FIG. 2.

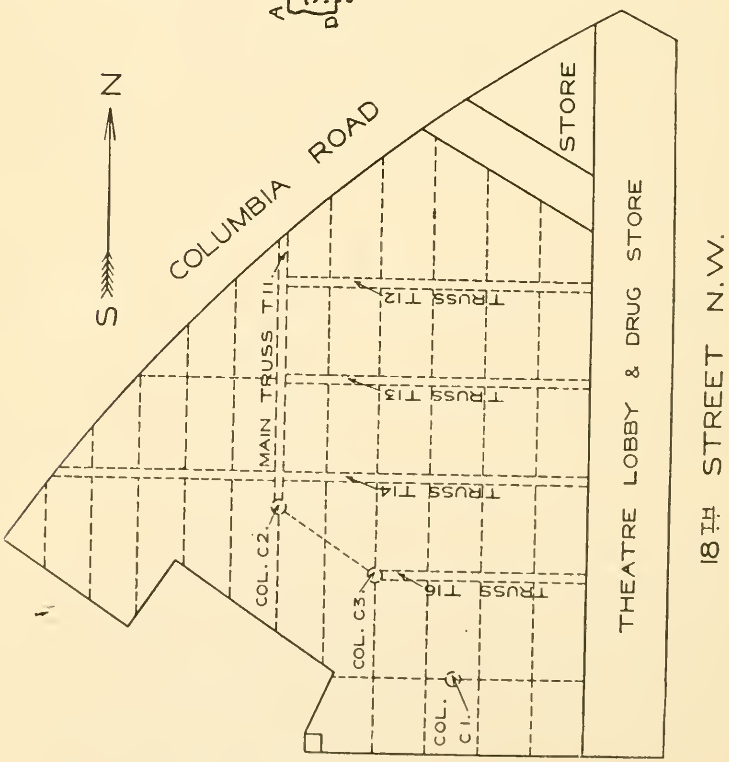


FIG. 1.

A paper was read before a large engineering society on the subject of the Knickerbocker Theater failure and the Salina, Kansas, Masonic Temple fail-

ure. The Secretary of the society sent me a copy of the paper and invited discussion. I promptly sent a discussion, setting forth facts and arguments, which led to conclusions not in agreement with those of the author of the paper. The Secretary thanked me for the contribution and said this was the kind of material they wished to publish, and it would appear in an early issue of their journal. I waited for the contribution to appear but in the meantime the publication committee got in their work. My contribution was not published, and after waiting some months and writing I learned that "nobody knows what made these buildings fail." I was reminded of the story of a man who, after he had purchased a horse, found that it was blind. He remonstrated with the man who sold it and was met with the response, "Well the man that sold it to me didn't tell me that it was blind, so I thought he didn't want it known." A few days before I wrote this I received a letter from a prominent engineer and professor on the Pacific coast asking me why I do not bring up for discussion before the large engineering societies the typical faults in structural design that I claim are responsible for the vast harvest of failures. Another prominent engineer in London has just asked me where he can obtain any data that will connect the rodded columns with failures that I say must be laid to its door. The Salina Building was a rodded column structure. The engineer who wrote the paper referred to attributed its failure to quite a different cause, though a few years ago he denounced the rodded column in the most scathing terms as being a wreck breeder and totally unfit to be used in a structure. The cause given for the Salina, Kansas, failure was sinking of forms. A large part of the wreckage was on the other side of a line of columns from the portion of the building where the forms are alleged to have sunk.

CHAPTER XVII

FOUNDATIONS

LATERAL FLOWING OF SOIL

A frequent cause of failure in foundations is the lateral flowing of the soil beneath a footing. This may be the result of footings being an insufficient depth below the surface, or it may result from excavation in the neighborhood of a building. The walls of the Homeopathic Hospital in Pittsburgh (See *Engineering News,* June 11, 1908) sunk because of three reasons. First, the pressure on the soil was too great; second, the footings extended but a short depth below the cellar floor; third, the clay soil on which the footings rested was allowed to be water-soaked and its bearing power thus greatly diminished. The moisture in the clay aided very materially in allowing lateral flow.

The lateral flowing of quicksand is especially to be guarded against. Retaining walls and embankments sometimes cave in with great destructive force by reason of dredging operations many feet away. The quicksand flows in as it is dredged out, the foundation is undermined, and finally a cave-in results. When a dredged hole continues to fill up in this manner, operations should be suspended, if there are any structures or embankments near.

Some buildings in Chicago sunk because of excavation for the freight tunnels in the neighborhood of their foundations.

One means of preventing the flow of soil when neighboring excavation jeopardizes the foundation of a building is to drive sheet piling around the footings to be protected. Another method is to use caissons and air pressure.

CONCENTRATED LOADS ON MASONRY

A frequent cause of failures in rubble and brick walls and piers supporting columns is neglect to provide spreaders for the concentrated load of the columns.

A building in Pittsburgh collapsed because the first story division wall had been removed and the columns supporting the upper stories in the place of this wall were placed on the rubble cellar wall without spreading beams. The investigation attributed the failure to "poor founda-

tions," though attention was specifically called to this structural blunder.

The cast iron bases of a number of columns in a brewery building in St. Marys, Pa., cracked because they were laid on rubble piers and failed to receive a uniform bearing. Spreading beams or a concrete pier would have prevented this failure.

In *Engineering Record,* Dec. 17, 1898, there is a description of some building columns which crushed into brick piers because the load was not properly spread over the surface of the pier. It is manifest that a brick pier should not be loaded with a heavy concentrated load, without ample provision for spreading the load over sufficient surface to reduce the pressure to safe limits.

In *Engineering News,* Oct. 19, 1911, page 489, there is a description of a building failure that occurred in Boston. A column supporting 40 tons crushed into the brick and rubble pier which was intended to support it. The large stones of the rubble portion of the pier were placed on the outside for appearance and the middle part, where the strength was needed was filled with small stones.

BLAME OF FAILURES OF DAMS WRONGLY PLACED OF FOUNDATION

Many failures of dams have been attributed to faulty foundation, when the fact is that the dams have been totally inadequate to remain in place against under pressure, which diminishes enormously the stability of the dam. When once under pressure and the pressure on the upstream side of a dam combine to raise a dam the slightest amount from its foundation, a fissure is created between the base of the dam and the soil. This water under high pressure, of course, washes out the soil, and the impression is created that the foundation has failed. This is not a foundation failure.

CHAPTER XVIII

FAILURES IN PIPE LINES

BENDS IN PIPE

A number of failures have taken place due to lack of provision for the longitudinal force in a pipe line carrying fluid under pressure. The examples where such failures occur are in pipe lines where the joints have little or no tensile strength. Leaded joints in cast iron pipe are frequently pulled apart where sharp bends in the pipe occur, or even if the bends are not sharp; and if the pipe is not held in line by the fill around it, it is apt to open up in the joints and fail.

A failure of this sort is described in *Engineering Record,* Sept. 25, 1915, p. 390. A number of other similar failures are referred to in this same volume of the *Record.* Sometimes pipe lines will give way where they issue from the pumping station and where a sharp turn in alignment occurs.

The remedy is to anchor the parts of the pipe together across the joints where longitudinal pressure or tension exists. Sometimes at such joints heavy concrete piers are built around the pipes on each side of the bend. An analysis of the forces existing on the cross-section of the pipe should be made, and these forces should be taken care of by adequate means.

CONCENTRATION OF PRESSURE IN ARCHED PIPE

Another type of failure in pipes occurs where in a curve in the line there is concentration of pressure at one point. Arching of the pipe or expansion creates great pressure on this point, and this pressure breaks out a piece of the pipe, or it may split the pipe. Cast iron pipe is particularly susceptible to this type of failure, because cast iron is brittle and not tough. The remedy against this type of failure is, of course, to avoid close contact between bell and spigot, particularly where the line of the pipe is arched or curved.

PIPE TOO THIN TO HOLD CIRCULAR SHAPE

A number of failures of steel pipe lines have taken place by reason of the fact that the shell of the pipe was not stiff enough to withstand the flattening effect of the water content or the external pressure of the air or the combination of these two effects. Such failures take place where the internal pressure of the water is small, or where, as in the case of a siphon it is nega-

tive. The latter condition requires a good stiff shell, particularly if full atmospheric pressure may be exerted on the outside of the pipe. The failures have been in pipe lines laid on the ground or only partially aided by trench or saddles on the under side.

One of these failures is described in *Engineering Record,* Apr. 18, 1914, and in *Engineering News,* Mar. 12, 1914. A rupture occurred at the bottom of the valley and the rush of water created a partial vacuum in the pipe on the slopes, even though the high ends of the pipe were open. The pipe was of $\frac{1}{4}''$, $\frac{5}{16}''$ and $\frac{3}{8}''$ plates and 10 feet in diameter. The pipe of the two smaller thicknesses collapsed, while the $\frac{3}{8}''$ pipe did not collapse. Water was forced into the pipe and it rounded out again. This pipe was only partially bedded in a trench.

A pipe 14 ft. in diameter, made of $\frac{5}{16}''$ steel with 6″x4″ angle rings at the circular joints and supported on two lines of concrete piers six feet between the lines and spaced at 14-foot intervals, did what would be expected of it. It flattened out at the first filling with water. (See *Engineering News,* May 1, 1913, p. 109.) It was the weight of the water and not external air pressure that caused this pipe to collapse.

An account is given in *Engineering News,* July 27, 1911, p. 112, of the collapse of some steel lock bar pipe. The sizes of pipe were 44″ in $\frac{1}{4}''$ steel and 52″ in $\frac{1}{4}''$ to $\frac{7}{16}''$ steel. Part of the pipe line collapsed under external air pressure. The pipe was saddled a little by earth, but it was not backfilled.

Of the three cases above cited the first two are manifestly examples where the pipe lines would not withstand any external air pressure. Pipe of such proportions should not be laid where the hydraulic gradient is apt to fall below the level of the top of the pipe. Internal pressure is necessary to hold such pipe in shape. A trench and backfilling would be very desirable, if not essential, for pipe of these proportions. The third example is of pipe that would about reach its ultimate strength under one atmosphere of external pressure. Failure of pipe under this condition would be similar to the elastic failure or buckling of a slender column, hence a factor of safety but little more than one is sufficient. A long steel pipe to withstand one atmosphere of external pressure, if it be saddled for about one-third of its circumference should have a thickness at least one-hundredth of its diameter.

CHAPTER XIX

EQUIPMENT FOR HANDLING LOADS

There are many failures in equipment used for handling loads. Generally these failures are due to faults in design or, what amounts to the same thing, the overloading of equipment, thus compelling it to carry loads not intended to be handled by it. However defects in materials are sometimes responsible for failures: also improper handling of equipment may result in bad accidents.

DAMAGED STEEL CABLE

I was called in on a case where a steel cable used in a hoist dropped the cage when loaded to only a fraction of the safe capacity of the cable. It was found on examination of the cable that it was heavily scored for a length of about two feet. The conclusion reached was that the cable had jumped the sheave, or a bolt or bar had dropped between the sheave and the cable while a load was being lifted or lowered at some time previous to the accident. This weakened the cable by destroying a large part of the strength of each strand. With each successive strand of the cable weakened by this scoring along the side it required only a fraction of the nominal capacity of the cable to tear it apart.

Kinks in cables are apt to weaken them, hence the importance of seeing that kinks are prevented from forming as the cable receives a load.

SMALL SHEAVES

Small sheaves, less than 30 or 40 times the diameter of the cable, are detrimental to the life of steel cables, though small sheaves are very common in derricks. Permanent installations should have large sheaves. It is particularly important, where sheaves must be located near one another and at right angles to each other, that such sheaves be of extra large diameters; as it is the bending of cables around sheaves that wears them out, and bending in two planes is particularly harmful.

Sheave pins are frequently of small diameter, and they frequently bear on cheek plates that are so wide apart that the pins are overstressed in bending.

THE AJAX CRANE FAILURE

On Dec. 7, 1914, the large floating crane Ajax at Panama Canal failed.

This failure is described in *Steel and Iron,* June 15, 1915, and in *Engineering News,* May 27, 1915. Quoting from *Steel and Iron,* "The consensus of authoritative opinion from the view point of the Panama Canal is that member No. 29 was the first member to fail, and that this failure was due, not to deficiency in sectional area, but to insufficient provision of latticing, batten plates, etc., to tie the two parts of the member together."

Fig. 1 shows this crane with member 29 marked.

The following is quoted from my letter published in *Engineering News* May 27, 1915.

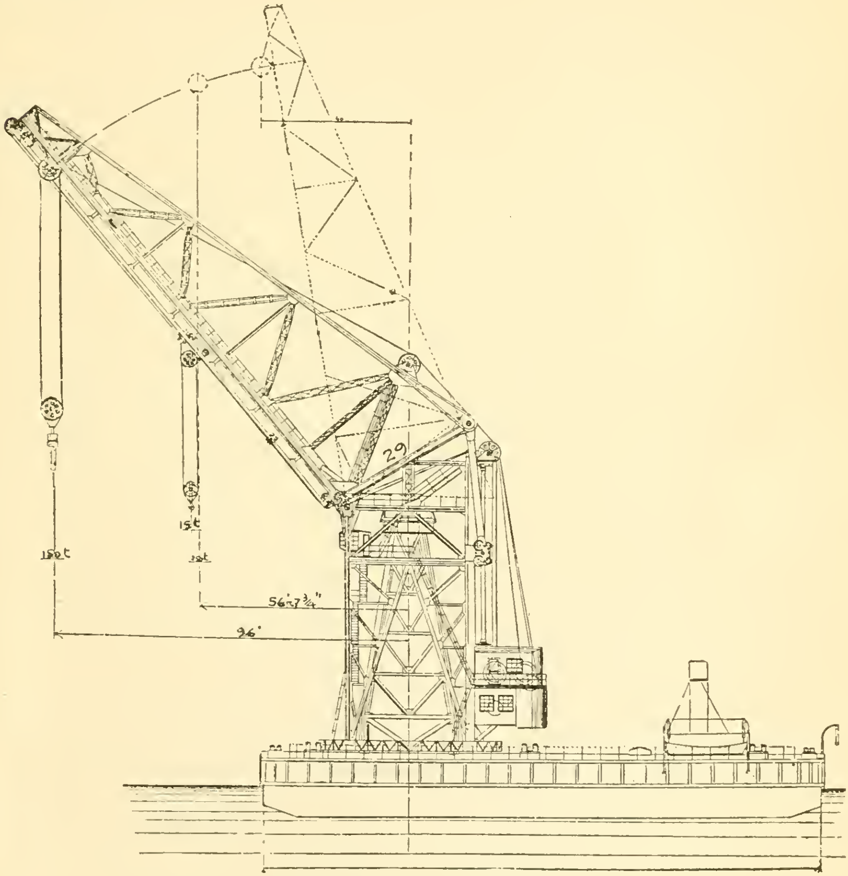


FIGURE 1.—SIDE ELEVATION OF FLOATING CRANE "AJAX."

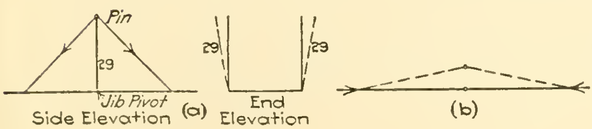


Fig. 2

"In my judgment this failure was not due to weak latticing in member 29. Heavier lattice bars might have changed the nature of the failure of the member, but could scarcely have prevented it.

"This failure is one piece with some others, among which is the Buffalo Pumping Station. In that failure the two compression members of the trusses (the top chords) were not held in any way at the peak. The sketch indicates at (b) what happened. The free ends of these members were pushed out of line, and of course the trusses dropped. Every truss built this way dropped; those that were held at the peak stood up.

"At (a) Fig. 2, the sketch indicates typically the condition in this crane. The members 29 were not held against lateral displacement. In this condition it was easy for these members to assume positions indicated by dotted lines. In this position the lattice would be tremendously overstrained, not because it is weak, but because the frame is improperly designed. The member, in crumpling would naturally snap off the lattice bars, but this is merely an incident in the collapse; a member very much stiffer in the latticed portion would have failed, but the failure would have taken a different shape.

"The designer of the reinforcement evidently realized that heavier lattice bars would not overcome the weakness for he used heavy stay diaphragms at the end near the jib pivot, 14 ft. long in planes normal to the frame. This very greatly improves the strength of the frame and compensates for the upper end of the member being free.

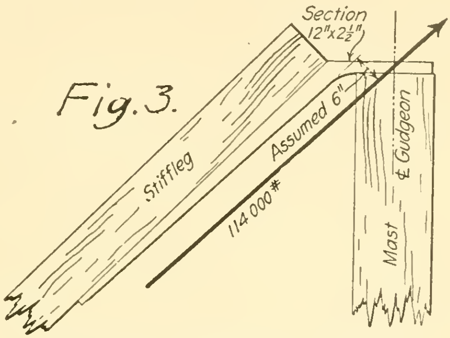
"Deductions as to the stresses in lattice bars based on the failure of a member such as this are of little value, because the frame and the member are improperly designed. This is not a member held at the ends, as nearly all compression members are. In a member free at the end where the load is applied the lattice bars have quite a different office from that which those in an ordinary compression member are called upon to perform.

"I do not believe there is anything in the way of test or theory that will show the need of provision in the lattice system for a shear greater than about 1% of the direct stress in ordinary compression members. Some tests were made on members having very thin webs that violated the ordinary rules of design of compression members. These showed a greater shear than 1%, but this was for the very reason that the members were improperly designed. The buckling of thin webs would put undue stresses in lattice bars, which would not occur in properly designed members. The same experimenters who found the high stresses in these lattice bars of the improperly designed member found scarcely any stress in lattice bars of a properly designed bridge member in service."

FAILURE OF A DERRICK GOOSENECK

The gooseneck of a derrick failed because the bending stress on the same was too great. A letter of mine appears in *Engineering Record,* Oct. 2, 1915, p. 425. Fig. 3 is from that letter. It is seen that there is a bending moment of six times 114,000 or 684,000 in.—lb. This on the 12″x2½″ plate would mean 54,700 lb. per sq. in. on the steel. To quote from the letter, "If the gooseneck were a tight fit on the gudgeon pin, the latter might help it out by taking a part of the moment, but this would be a precarious thing to depend upon. Attention is called to it only as a

Fig. 3.



GOOSENECK UNDER BENDING STRESS

possible explanation of why the gooseneck did not fail sooner. Excellent quality in the steel would not save for very long a piece of equipment subject to stresses like this." Some things about the strength of equipment used for handling loads need closer analytic scrutiny than they are receiving.

STRENGTH OF EQUIPMENT FOR HANDLING LOADS

Re-printed from Proc. Eng. Soc. W. Penn., Jan., 1914.

By Edward Godfrey

Accidents frequently happen where heavy loads are being lifted. Many of these accidents are due to weak parts or details of the equipment used in handling loads, and they could be avoided by proper care in the selection of these parts or a proper design of the details. Very frequently but little thought or care is given to these matters, the part being selected by rule-of-thumb methods and the details being designed in the same manner.

If permanent structures received no greater care in their design than many of the tools for handling great loads, it would be hazardous to take a ride over a bridge in a train. Why should not the same degree of safety be demanded where a few lives are jeopardized as where a large number of lives are concerned?

Hooks: Some time ago the writer was called upon to pass upon the safety of a piece of equipment that was purchased as a 12-ton derrick. The first thing he found was a 6-ton hook. Other parts, while not so weak, were far from figuring up to their requirements.

It is not difficult to figure out the safe load that a hook of any given shape and dimensions can sustain safely. The critical section is a horizontal one just beside the opening of the hook.

In any of the sketches in Fig. 1 the bending moment at this critical section is Pa. If in the case of a plain round rod, curved into a hook, we assume that the inner diameter of the hook is twice the diameter of the iron, the bending moment is $\frac{3}{2} Pd$. Using 10,000 lb. per sq. in. as the safe fiber stress and the section modulus of a circular section, $0.0982\ d^3$, we have, when direct tension is considered, $P = 600\ d^2$.

From this we see that a ½ inch round rod of ordinary steel in a hook of this proportion would sustain safely a suddenly applied load of about 150 lb.

A one inch rod would be good for 600 lb.; a two inch rod for 2400 lb.; a three inch rod for 5400 lb.

If the rod is flattened at the side of the hook, the same amount of metal will be stiffer, and the hook will be more efficient. Assuming that the iron is flattened out at the side of the hook in the shape of an ellipse with the long diameter twice the short diameter and that the inner diameter of the hook is equal to the long diameter of the ellipse, we have the following:

Diameters of the ellipse of area equal to area of circular section of diameter d are $1.414\ d$ and $0.707\ d$.

Section modulus of same ellipse $= 0.139\ d^3$ Moment $Pa = 1.414\ Pd$.

Adding direct tension to extreme fiber stress for bending, we have for a total fiber stress of 10,000 lb.:

$$P = 870\ d^2$$

Hence a one inch rod would make a hook of these proportions good for 870 lb.; a two inch rod for 3480 lb.; a three inch rod for 7830 lb.

The foregoing figures are given to show approximately the capacity of hooks made of round iron and the advantage of the simple expedient of flattening the iron at the side of the hook. For any special hook made of round iron the capacity can be worked out by using the actual diameter of the inside of the hook and of the iron used.

Standard hooks are made with dimensions and proportions as shown in Fig. 1 at "*C*." The thickening of the cross section near the inner part of the hook has the double advantage of bringing the center of gravity of the section closer to the line of application of the load and of increasing the area of metal resisting tension. It is in tension of the extreme fiber at the section of maximum bending that a hook is more apt to fail than in compression, and a tension failure is more disastrous, as the hook would not bend but snap in two without warning. Hence the importance of added tensile strength.

This style of hook was designed and its proportions worked out by Mr. Henry R. Towne, and is described in his treatise on Cranes. The writer has taken these dimensions and worked out the section modulus of the hook as well as the location of the center of gravity of the section. The section modulus for tension was found to be 0.15 times the cube of the diameter of the iron used in making the hook. A few of the given capacities of these hooks are given below:

Diameter of iron	5/8	3/4	1 1/4	1 3/8	2 1/4	3 1/4
Capacity in tons	1/8	1/2	1 1/2	2	5	10

The writer has tried a number of these hooks, and he finds that the extreme fiber stress is close to 18,000 lb. per sq. in. This means that these hooks, in order to be safe for suddenly applied loads, should be made of high carbon steel and not of the ordinary grade of structural steel.

Some special hooks are in use, such as those for picking up girders by their flanges. If we assume that the lines OA and OB of Fig. 2 make angles of 60 deg. with the vertical, the force along each line is equal to the load lifted, or P. The critical section is at the bend, and the bending moment is Pa. Assuming that a is twice the thickness of the bar of which the hook is made, we find that at 10,000 lb. per sq. in., when direct tension is considered,

$$P = 770\, bd$$

Two by one-half inch bars in hooks of these proportions would be good for lifting 770 lb.; four by one inch bars for 3080 lb.; etc.

As in the case of the other hooks, the proportions may not be the same as those assumed here. In any given case these may be measured and the capacity of the hooks worked out by the method given.

The unit stress assumed in the foregoing is based on the use of ordinary structural steel. This unit stress would allow for the shock of a suddenly applied load. If high carbon steel were used, the unit stress could be higher and the capacity of the hooks would be correspondingly higher.

High carbon steel is often used for hooks because a smaller hook may be used in the stronger material. If the fiber stress never exceeded the elastic limit of the material, high carbon steel would be entirely safe for hooks. The load on hooks, however, is apt to be quite variable and to be dropped some distance in the handling. If the fiber stress equals the elastic limit, a number of applications would break the hooks. In softer steels the hook would probably be bent and thus give warning, but in high carbon steel it would snap off short without warning. This is the danger of using hard, brittle steels where the loads are apt to overstrain the steel. Soft steels are not free, however, from this danger of sudden and sharp breaks when subject to repeated excessive stress.

Hooks are sometimes annealed, after they have been in service for some time, to restore the metal. The common notion is that steel crystallizes in service and that annealing restores the structure that is injured by this crys-

tallization. This is one of the half-truths that give rise to much misapprehension. Steel can be made to give service indefinitely without any deterioration; furthermore, it is impossible to change the crystalline structure of cold steel.

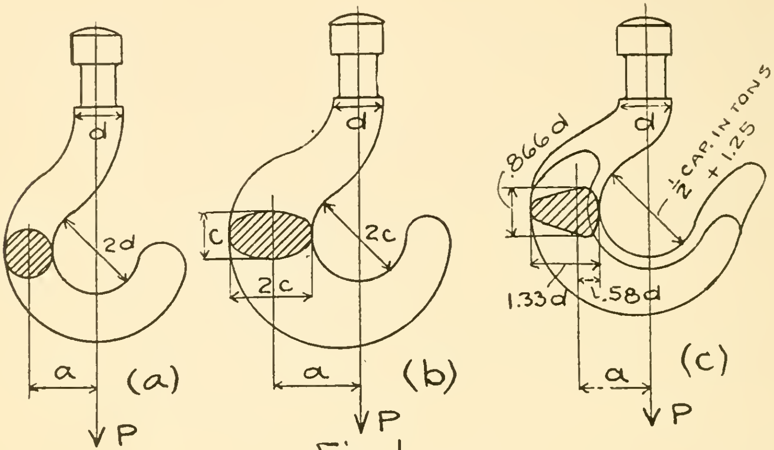


Fig. 1.

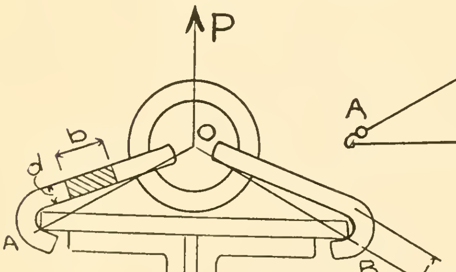


Fig. 2.

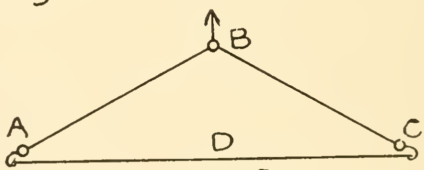


Fig. 3.

The condition under which steel will remain permanently uninjured is that the fiber stress never equals or exceeds the elastic limit. Repeated application of fiber stresses near to or in excess of the elastic limit will rupture the steel eventually. Unfortunately equipment for handling loads is very often made of such size that the loads handled, when shock is considered, are sufficient to stress the steel close to the elastic limit. The result is that such parts as hooks and chain are apt to fail in service, and every precaution is needed to avoid this.

Steel that has been badly treated by applying strains close to the elastic limit is rendered brittle. Annealing restores to some extent at least, the toughness of the steel and would often forestall failure.

The idea that steel is rendered crystalline by shock is a mistaken one. Steel is crystalline under any circumstance. If it is broken suddenly, it will show up the crystals more clearly in the fracture, whereas, if the same piece

were pulled apart slowly in a testing machine the crystals would be drawn out like fibers. Accidental breaks are almost always sudden ones, they show crystalline fractures, hence the common idea that steel crystallizes in service.

Rings: Rings are necessary on the ends of chains to throw over hooks and to pass the chain through in forming slings. Closed rings can be of much smaller diameter iron than open rings, but round rings must be of larger iron than the links of chain, which are usually of oval shape. Also the rings being of larger diameter than the links of the chain require further increase in the size of the iron for equal safety.

If a circular ring whose diameter is D, made of round iron of a diameter d, have a load of P applied in the manner in which it would be used in a chain, the bending moment at the point of application of the load is

$$M = 0.1592\ P\ D$$

and at the side of the ring

$$M = 0.0908\ P\ D$$

There is a direct pull at the side of the ring, the unit stress of which would be added to that found by this last bending moment, but in a ring of ordinary proportions this would not be sufficient to make the extreme fiber stress as great as that at the point of application of the load. Hence the greater moment is the criterion. Equating this to the section modulus of the iron ($0.0982\ d^3$) and using 10,000 lb. per sq. in. of extreme fiber stress, the safe capacity of the ring is

$$P = \frac{6170\ d^3}{D}$$

Hence a ring of ½ in. diameter steel, 3 in. in diameter, is good for 260 lb.
¾ in. steel in a 4 in. ring is good for 650 lb.
1 in. steel in a 6 in. ring is good for 1030 lb.
1¼ in. steel in a 7 in. ring is good for 1720 lb.
1½ in. steel in a 8 in. ring is good for 2600 lb.

These values are very low, for quiescent loads, they could be twice this amount or more.

Oval links are stronger than round ones because the bending moment is less. Without going into the exact theory giving the bending moment on an oval ring it is close enough to consider that the strength of an oval ring is equal to that of a round ring of a diameter equal to the short diameter of the oval ring.

The writer knows of no standard giving the safe strength usually attributed to rings, so that he cannot make any definite statement as to the degree of safety usually employed. He has seen rings break in service, which is a good sign that they were overtaxed in that service.

Chains: The safe loads which chains may carry are usually taken as one-third of the ultimate strength of the chain under test.

The proof strain is usually one-half of the ultimate strength of the chain. This load is usually applied to the chain after its manufacture. It is by no means a safe load for the chain, though it is sometimes considered such. Applied to the chain once, it elongates the links somewhat, and its application will usually detect any badly welded links. But repeated application of this load would in time break the chain, as it strains the fibers beyond the elastic limit.

When chain is strained to the point of rupture in a testing machine, the links elongate and become straight and parallel along the sides. The failure is then one of direct tension on the steel, and the bending strains of the original link are not present. For this reason the ultimate strength of steel or wrought iron chain is not in itself a proper criterion of its safe load.

There is another way that chain can be broken. This is by the process of repeated application of a load that will strain the extreme fibers of the metal beyond the elastic limit. The links need not be appreciably distorted by this, but the metal is gradually made brittle and will eventually fail at a load equal to or less than that which it has repeatedly carried. The break will be short, showing a crystalline fracture, and the majority of observers will say that the steel was crystallized in service, or was a poor grade of metal, or was burnt. All three answers are wrong. This false interpretation of failures that result from overloading equipment is so common that it has become engineering second nature to incorporate it into practically every report on such failures. Rings and hooks and bends and chains all fall in the same class. Men reason that because the load is carried once, or a number of times, or because the ultimate static load to rupture the thing is three or four times that which it is carrying, it is perfectly safe. When failure takes place they say bad steel or crystallization. There is no more invidious error than this. It has taken hold of the engineering profession, and from the writer's experience in trying to root out sundry other errors he believes it bids fair to abide for many years.

The standard safe loads used on chain in one bridge shop are shown in the following:

Dia. in inches	$\frac{3}{8}$	$\frac{1}{2}$	$\frac{5}{8}$	$\frac{3}{4}$	$\frac{7}{8}$	1	$1\frac{1}{16}$	$1\frac{1}{8}$	$1\frac{3}{16}$	$1\frac{1}{4}$
Capacity in tons	1	2	3	5	6	8	10	11	12	14

This is in agreement with common practice. The loads are about one-third the ultimate strength of the chain.

By applying the formula for round rings previously given herein, namely

$$P=6170\frac{d^3}{D}$$

and assuming the short diameter of a link to be $2\frac{1}{2}$ times that of the iron ($D = 2\frac{1}{2}\, d$) we find that

$$P = 2500\, d^2$$

Solving this we find that for

$\frac{3}{8}$ in. chain the safe capacity is 350 lb.
$\frac{1}{2}$ in. chain the safe capacity is 620 lb.
$\frac{3}{4}$ in. chain the safe capacity is 1400 lb.
1 in. chain the safe capacity is 2500 lb.
$1\frac{1}{4}$ in. chain the safe capacity is 3900 lb.

It will be seen that these values are exceedingly low, something like one-seventh of the commonly used "safe" loads. Even if doubled, that is, if 20,000 lb. be taken as the extreme fiber stress, the values are still very low. There is one allowance that could be made in this theoretic strength of a chain, namely a reduction in the lever arm because of the fact that links fit together to some extent and do not bear on a point. Making all such allowances, however, it is plainly seen that the commonly used safe loads on chain are such as to strain the fibers to the elastic limit. This is why chains get brittle and break. This is why they need frequent annealing. This is why so many coroners' juries have deliberated over a little piece of "defective," "burnt," "crystallized" steel.

Stud link chain is stronger than ordinary link chain because the studs prevent the links from flattening and diminish the bending moment. The advantage of stud link chain over ordinary chain is much more than that shown in ultimate tests because of the diminished bending moment, something that does not show up in ultimate test loads, but does show up in a large number of applications of a supposed safe load.

Chain that is safe for lifting a certain load may be overstressed in lifting that load by improper hitching.

In Fig. 3 if the spread of the chain is such as to give an angle of more than 60 deg. with the vertical, the stress in the two parts of the chain will be greater than the load lifted. It is not difficult to determine practically, when the angle is too flat. If the parts of the chain are not equal to twice the vertical height *BD*, the force triangle is flatter than an equilateral triangle, and the strain on the two parts of the chain is greater than the load lifted. A load of the capacity of the chain should not then be lifted.

Chain is usually made either of wrought iron, or soft steel that is capable of being welded. Wrought iron chain is better than steel chain of the ordinary quality, because it will stand weather better, and because wrought iron welds are more reliable than steel welds. Also wrought iron links are less apt to break short; they will bend and give warning of approaching failure.

There is not much difference in the strength of wrought iron and soft steel chain. The grade of steel used in the manufacture of the latter has an ultimate strength of but little more than 50,000 lb. per sq. in., which is about the high limit of wrough iron.

Chains as well as rings and hooks are commonly annealed to restore the toughness of the steel or iron. This annealing usually consists merely in

building a fire and throwing the chain into it and heating it red hot and then allowing it to cool.

The toughness of the metal seems to be restored by this process, for chain that is quite brittle can be rendered useful by this annealing. The fact that the chain is brittle and needs to be annealed indicates that it has been subjected to service that strains the fibers close to or above the elastic limit. This common practice of annealing chain, as well as the frequent breaks that occur, are the outcome of the still more common practice of overloading equipment for handling loads.

Cables: Hemp and sisal ropes and steel cables are much used in handling loads. As a rule there is more safety in the use of cables than in chains or hooks. Cables loaded excessively will stretch out appreciably and give evidence of their distress. They will not last long, however, especially with the small sheaves that it is customary to use in hoisting equipment. An advantage is that they seldom break suddenly and the wear or deterioration is plainly visible on the surface in broken wires.

The ultimate strength of a few sizes of manilla rope are given below:

Diameter in inches	¼	½	¾	1	1½	2
Strength in lb.	780	2250	5000	9000	22,500	39,000

Sisal rope is about three-quarters as strong as manilla. The strength of ropes is quite variable because of the different qualities of material used in their manufacture. The factor of safety should be five or six for a good life of rope.

The ultimate strengths of a few sizes of steel cables made of 200,000 lb. steel, are given below:

Dia. in in.	¼	⅜	½	¾	1	1½	2
Load in lb.	4800	10,000	17,600	38,800	68,000	144,000	248,000

These breaking loads are for cables having hemp centers and with six strands of 19 wires each of so-called crucible steel. The writer is informed that there is very little steel cable made in this country of crucible cast steel. The material generally used is an imported open hearth steel.

The proper safe load on a steel cable is governed somewhat by the use to which it is to be put. If it is to sustain a steady pull in a stationary position a factor of safety of three would be sufficient. If it is to be passed continually over small sheaves and subject to varying loads, the factor of safety should be five or six. In general the larger the factor of safety used the greater the life of the cable.

It is difficult to make an end hitch on a steel cable that will stand the strain required to break the cable. The way it is frequently done is to bare and broom the ends of the wires, pickle them, and then put this prepared end in a conical socket and pour melted zinc around it. It might be reasoned from this that as this is the strongest end detail it is the proper one to use on the cable in practice. This is not the case, however, a better end detail is to

loop the cable around a thimble and use several clamps to bolt the free end to the cable itself. This kind of hitch is probably not so good in a testing machine, because at the ultimate load of a cable it is drawn down in diameter and would slip through clamps. At the working load of a cable this would not take place. The socket detail is uncertain. It is impossible to tell whether the wires are held firmly in the metal without pulling the joint apart and destroying it. Also there may be unequal stress on the several wires as well as some tendency to anneal due to contact with the hot metal. It can be readily seen whether the other end hitch is properly made or not.

As will be more fully pointed out under sheaves, the sheaves and drums for cables should not be too small. Curving the cable to a short radius while under stress is detrimental. The outside wires are thereby overstressed and the life of the cable is shortened. It is particularly bad when the drums or sheaves are close and so located as to bend the cable in different directions, hence it is important in the proper use of cables to avoid such conditions in the design of equipment.

Cables should be lubricated, not only to prevent rust, but to allow the wires to slide on each other so as to diminish the strains due to the bending of the cables. Sometimes a mixture of linseed oil and pine tar is used as a lubricant, sometimes mica axle grease is used.

Cables in service gradually wear out by breaking one after another of the wires. These breaks are on the outside of the cable where they can be seen and counted. If these breaks are generally distributed along the cable, the reduction in the strength of the cable is not great, unless the breaks are quite numerous. Mr. S. Diescher in the Proceedings of the Engineers' Society of Western Pennsylvania, Volume 12, states that when it is found that within the length of the pitch of a strand about 40 per cent of the wires are broken, it is time to discard the cable. There are 114 wires in the ordinary cable. If 46 of these are broken in the length of the cable in which a strand makes a complete whirl, it is time to throw away the cable. This count can readily be made. It should be made at several points along the cable, and the state of the cable should be judged by the worst condition, not by the best or the average.

Sheaves: It is the general rule that the sheaves for ropes and cables should be of a diameter 30 or 40 times that of the cable or ropes. While this rule is observed in stationary sheaves, for elevators, inclines, etc., it is very generally ignored in equipment for handling loads. This is not a very serious matter however, as it simply means that cables will wear out in a shorter time. Thirty inch sheaves on a derrick using ¾ inch cable would be very much in the way. The rule can scarcely be adhered to in ordinary equipment.

In the matter of the sizes of pins in sheaves there is room for im-

provement in common practice. If the ordinary methods of figuring bridge pins were applied to sheave pins, many of them would be found to be very highly overstressed. Many designers figure such pins for shear only. This is a mistake. The bending moment is the governing factor in a pin, and in fact the only one that needs to be considered.

The writer has checked the designs of many derricks and has talked with the designers. There is a general disregard of engineering principles, particularly in the matter of bending moments on pins, exhibited both by the designers and by their work. Pins 1¼ to 2.0 inches in diameter are the common standards. The extreme fiber stresses calculated in the usual way are often in excess of the ultimate strength of the steel, just like the highway bridge designs of 25 years ago.

When pins are in iron or steel bearings they can be considered as having high bearing pressure, thus shortening the lever arm for bending, but when they are in wood, the high pressure would crush the fibers of the wood. It is important in such cases that the pins be of extra large size, so that they will not bend.

Derricks: In addition to the parts already mentioned, all of which are used in derricks, there are other parts in derricks that need attention from the standpoint of strength of design; the mast, the boom, the guys, the stiff-legs, the goose-necks, the gudgeon pin, etc.

In a guyed derrick the mast must be high enough to allow the boom to pass under the guys. In many stiff-leg derricks the boom is twice as long as the mast. Derricks with masts 30 to 35 ft. long and booms 60 to 70 ft. long are not uncommon, such derricks need special care on account of the long boom, and the problems entailed thereby. One of these problems is the support of the long and slender boom. Frequently the dead end of the boom line is hitched to the boom about at the middle of the boom to support the weight. The writer sees no objection to this so long as the strain is not too great for the weight of the boom.

Hog rods with one or two intermediate posts are very useful in reducing the unsupported length of a boom. These should be used where slender booms are employed. It is best to use them on all four sides of the boom.

In designing a long boom the ordinary column formula is of no use. The straight line formula for columns, while it is excellent for bridge members, fails when attempt is made to use it for slender columns. The Gordon-Rankine formula, with the constant usually given, is unreliable and unsafe when used for slender columns. The Euler formula, when properly interpreted, that is, when it is understood that the formula gives the absolute ultimate load that a column can sustain, is the proper formula for slender columns. The common idea that the

Euler load is one that a slender column can hold in equilibrium is totally false and gives rise to misapprehension.

If the modulus of elasticity of wood be taken at 1,500,000 the unit stress in a column at ultimate load is:

$$K = 1{,}200{,}000\frac{d^2}{l^2}$$

where K is the stress per square inch and d and l are the minimum width and the length respectively of the boom, both in inches.

For example a 14 by 14 inch boom supported for its own weight, of a length of 70 ft., will have an ultimate strength of 330 lb. per sq. in. For safe load the factor of safety should be at least two. This boom would then be good for 165 lb. per sq. in. of load or 32,300 lb.

It is a remarkable fact that the boom mentioned in the last paragraph, if supported freely in a horizontal position will have an extreme fiber stress from its own weight of nearly 900 lb. per sq. in., and yet it will not fail from this stress. It will, however, be very greatly weakened as a compression member. This boom is one that failed in a derrick in this city about eight years ago while lifting a load of 18,000 lb. The unit stress from this load was 170 lb. per sq. in., which is just about the allowed safe load, but the weight of the boom was not supported, and in its inclined position the extreme fiber stress from the weight was 590 lb. per sq. in.

The strength of slender compression members under lateral loads is something that has been neglected in engineering literature. The writer will not attempt to go into this at this time except to point out that long booms should be supported so that their weight will not be so large a factor in producing stress.

In Engineering News, June 26th, 1913, there is an account of a derrick boom that failed while lifting a load of only about one ton. The boom of this derrick was ten inches wide and its length was 65 ft. The ultimate strength of this boom is 200 lb. per sq. in. The boom was supported at the middle by the dead end of the boom line, and at the upper quarter point by two parts of the same line on a sheave. This double upward force at the upper quarter point being much in excess of the weight of the portion of the boom supported, coupled with the extreme slenderness of the boom, seems to have caused too great a bending moment on the boom, the endwise load being considered.

The base detail of a derrick is important, especially in a derrick having a long boom, on account of the large horizontal force against the foot of the mast. The writer knows of one case where a derrick was set on two 12 in. by 12 in. timbers. With a low boom the horizontal force was sufficient to rock these heavy timbers. The sheaves for the

boom line and the hoisting line were placed between these timbers. The remedy used was to bolt the timbers together, using spacing blocks.

The gudgeon pin is the pin at the top of the mast placed in a vertical position. The goose-necks, large iron straps fastened on the ends of the stiff-legs, pass over this gudgeon pin. This pin is sometimes too small in size and not firmly held at the extreme end of the mast, resulting in crushed fibers of the mast, or a bent pin. There should not be merely a ferrule driven in the top of the mast with the pin in a hole in the wood, but there should be a steel plate over the end of the mast in which the pin fits snugly, the plate being bent down over the sides of the mast, or a detail equivalent to this. Also the mast should be securely bolted a short distance from the end to prevent splitting.

Cranes: The various manufacturers of cranes have standard cranes for given capacities. These standards show the clearances necessary in a building, also the wheel spacing and the loads on the wheels as well as the size of rail. Roughly the load on a pair of wheels is about double the capacity of the crane. The wheel bases usually run from seven to twelve feet. A number of these standards are shown in the writer's book of Tables.

The maximum moment on a crane girder will occur when the center of span bisects the distance between the load near the center and the center of gravity of the system of loads on the span. With a single pair of wheels one wheel will be placed a distance from the center of span equal to one-quarter of the distance between the wheels. The maximum moment is:

$$M = \frac{2P}{l}\left(\frac{l}{2} - \frac{d}{4}\right)^2$$

where M is the maximum moment in foot pounds,

l is the span in feet,

d is the distance in feet between wheels,

P is the load on one wheel,

If d is greater than $0.59\ l$, one load at the center of span gives the maximum moment.

With four wheels that may all be on the span, one of the two inner wheels is placed a distance from the center of span equal to ¼ of the distance between the two inner wheels.

Traveling cranes or gantries are of many forms and present many problems in strength. The one of the very greatest importance is that of bracing. More wrecks have been caused by lack of bracing false work and erection equipment than from any other single cause, if reinforced concrete wrecks be excluded.

Timber travelers are usually well braced, but it seems that the larger

the traveler and the more the necessity for bracing, the less, relatively, is the provision against lateral sway.

Fourteen months before the Quebec bridge failure the writer tried to warn its builders against the danger of erecting it with an unbraced traveler. Practically all of the large structural wrecks in steel work have been due to lack of bracing, generally the entire absence of bracing, not merely insufficient bracing. And yet attempt was made to erect this record bridge with its members of enormous weight with a traveler that had absolutely no bracing between the side bents, more astounding, an expert, a Royal Commission, said absolutely nothing about this lack of bracing in the traveler, though public attention was called to this a few days after the wreck, and it was shown exactly how the failure took place, as it did, because this traveler was not braced and was pulled over, pulling the whole top system of the bridge with it.

Fig. 4 shows the writer's idea and theory as to how the Quebec bridge was pulled down. To the writer's knowledge this has never been controverted nor explained away. (This figure is given in Chapter IX.)

CHAPTER XX

THEORY SIMPLIFIED AND JUDGMENT ELIMINATED

Safety in structures is greatly advanced by simplifying the rules and methods of design and by reducing theory to its simplest terms. There are some complexities that are totally unnecessary for practical designing. They do not contribute to correctness, economy or safety in design. When the designer's work is simplified and the checking of the same facilitated, errors are less apt to be made.

For many years, in engineering papers, articles, letters, contributions to discussions, and reviews of books, I have contended for simplified theory and the elimination of many of the intricate methods of design that are proposed. In this chapter will be given some quotations from these published utterances and some summaries condensing the meaning expressed.

Many of the errors made by theorists are due to the fact that the one who handles the theory fails utterly to grasp the fact (in dealing with steel) that while steel is elastic it is not perfectly elastic, and it has a quality that is just as important—in fact frequently more important—than elasticity. This quality is best described by the term toughness. Because of the toughness of steel structural members and details will stand being subjected to stresses and conditions that would produce failure in a medium such as glass, which is almost perfectly elastic. When steel members and details resist stresses and moments which, according to the predictions of theorists, ought to cause failure, the theory is discredited; and because of this designers are apt to have a contempt for the theory and to disregard theory in some other respect, where such disregard may have a disastrous consequence. Designers should know, then, when the toughness of steel may be relied on in disregarding theory, so to speak, and when such reliance is fraught with danger.

In *Engineering News*, Jan. 27, 1916, p. 186, in a letter discussing Eccentric Details, I made the following statements:

"The theory of flexure presupposes a material that is perfectly elastic. It does not take into account the toughness or ductility exhibited beyond the elastic limit.

"By virtue of toughness, many structures persist in standing and carrying their load in spite of theoretical stresses that would cause them

to collapse. On the other hand, reliance on the toughness or ductility of steel gives rise to a large class of failures. Many members carry loads that theoretically ought to break them and that eventually do break some of them.

"In structural work columns are often milled out of square, and they bear on one corner or one edge. Theoretically the stress produced by this condition is much more than the ultimate strength of the steel, but the columns continue to carry their load without any apparent distress. Is the theory therefore at fault? Not at all. The theory tells us what would happen in a perfectly elastic material, and if steel were perfectly elastic, like glass, doubtless the columns would crush and fail. In the case of a badly milled column, if slenderness and deflection did not enter, at the corner where excessive pressure existed the metal would probably be peened down by the excessive pressure; little or no damage may be the result.

"Much space is taken up in engineering literature in describing the calculation of secondary stresses such as those due to the friction of rotation of members on pins and similar causes. Such calculations are practically of no value, because such stresses are of no consequence, and this is because of the toughness of steel. The kneebrace under a girder could be shown to have theoretically a tremendous stress due to deflection of the girder; and yet if that kneebrace were sawed in two, nothing would happen.

"As pointed out by Mr. Fleming, the addition of rivets to a joint, making it eccentric, may result in weakening the joint theoretically. Doubtless in a test this apparently weaker joint would prove stronger in this particular case, and such experimental demonstration would seem to nullify the theory. Here again the factor of toughness enters; also a further fact, namely, that rivets do not join metal so as to make the connected pieces like one piece. Under test there is a small slip in the rivets, and this brings about a readjustment of the stresses on the rivets. Thus a joint may adjust itself to adverse conditions and hold where the theory would predict failure. These facts are cited, not as justification for reckless design of riveted joints, but as explanation of why many recklessly designed joints do not fail. Proper design should take care of eccentric application of loads on riveted joints.

"Theoretically if a bar is doubled in width by adding to one side only, the member is weakened. Practically, if a test were made, the bar, if of steel, would probably be stronger in spite of the eccentric application of the load. If the addition were made by riveting on metal, the advantage might be even more in favor of the wider bar, for the reason that slip in rivets would probably allow practically the whole of the

original bar to come into play, with uniform intensity of stress. Proper application of theory would discover these facts. But the designer is not thereby justified in using eccentric members without due provision being made for the bending moments.

"In the case of tension members, especially those subject to repeated loading and eccentric stress, failure sometimes results after many repetitions of the load. This is particularly true of such things as chains and hooks. Chains and hooks that have carried many loads sometimes fail under the same load as one previously carried. Calculations will show that the load produces unit stresses equal to or greater than the elastic limit of the steel. The metal is rendered brittle by repetition of this strained condition, and failure eventually takes place. Contractors anneal their chain from time to time to counteract this brittle tendency. This is virtually a confession that the chain is strained beyond its elastic limit, else it would never need to be annealed.

"Permanent structures never need to be annealed, if they are properly designed and eccentric stresses taken care of. Theory is the proper thing to be guided by in this, and not test. Tests would have to be repeated many thousands of times to be conclusive."

One of the arguments used to bolster up a type of design that is otherwise indefensible is the statement that structures designed in that manner are standing. It is frequently used to justify types of reinforced concrete design many examples of which have proven complete failures. This argument was used to justify some practices in the design of steel bridges in Europe which are of questionable nature—practices that fortunately are avoided in American designs. The attempt was made to show that the European bridges because they are lighter are therefore more economical, inasmuch as they stand up and carry their loads (lighter loads than ours, however). In a paper read before the *Western Society of Engineers* in 1913 I defended American practice and in *Engineering and Contracting*, Dec. 28, 1921, answered the critic of American practice referred to in this paragraph. The following is quoted from that contribution:

"If American builders wanted to design with total disregard of engineering mechanics, they could also make light structures and get away with it, too, for there is a vast amount of their work, particularly in existing highway bridges, that is on that order.

"I have examined in detail a large number of bridges and reviewed the new designs of as many more, and it is astounding the things that are proposed and executed in steel. It is still more astounding the things that stand up and carry loads. Main chord pins in pin-connected trusses that are only large enough to be considered good sized bolts; lateral

rods that are attached to swinging floorbeams and with a hook to the end shoe, details that would take scarcely any stress; braces to top chords of pony trusses that are good for scarcely anything; main girders with one, two, or three rivet end connections; sidewalk brackets with great heavy flanges, and then the whole thing sustained by a ¼-in. web plate perforated with a line of holes.

"Considering corrosion, webs of main girders are found that can be perforated with the hammer; top flanges of stringers and floorbeams are found where the metal is nearly all gone; one I-beam stringer was cracked through the top flange and nearly to the bottom flange, still carrying street car loads.

"Nature is never freakish in the things that collapse. There is always a good, sound reason for every failure, if the facts could be known. Every structure is stronger than its designer has any right to expect. There are many freakish things standing, such as balanced rocks and a large number of bridges.

"There is a very wide margin between a well-designed bridge and a structure that will 'stand'; and it is often hinted that we are wasting money if we build the former when the latter would answer the purpose and do it for many years, if not indefinitely.

"Do we want to make structures that are just inside of the border line of destruction, or do we want to say to the public, domestic and foreign, that this structure is absolutely safe for all legitimate use and forever, if it is simply kept protected by a coat of paint?"

There are those who would reduce to a minimum standardization in the design of structural work on the plea that it destroys initiative and the exercise of the individual conscience. I have always taken the stand that the more structural designing is standardized and reduced to rule, and the more individual judgment is eliminated, the better will be the structures turned out. This, of course, presupposes that the rules are correct and can be analyzed (and are therefore different from many of the standard rules of designing in reinforced concrete); and of course it has relation only to the things that make for strength and stability. The following is quoted from a letter on this subject published in *Engineering News-Record*, Aug. 28, 1919.

"In checking plans and examining structures during a period of more than a quarter century, I have many times found errors in design and details that could not possibly have been perpetrated by designers if they had observed certain rules of design, some of them of the simplest nature. Many times I have in correspondence with designers had the utmost difficulty in convincing them that their designs were faulty and

actually dangerous—this because there was no rule published that covered the particular case in question.

"Your railway engineer of long experience need not worry about the individual engineer's lack of opportunity to exercise his conscience. There is no better way to exercise the conscience than to work strictly in accordance with a set of good rules. Furthermore, there is nothing more at variance than the consciences of a lot of different individuals, whereas the laws of stability and equilibrium are as constant and invariable as the eternal hills and care as little for the passing opinions of professional men, no matter who they be.

"Where would structural engineering be if it were divested of the rules of design that demand that unit stresses be so much for tension, so much for compression, so much for shear? Suppose every engineer should say that his conscience tells him that rivets are worth so much. All would be different; there would be confusion worse confounded. One man would use 10,000 lb. per square inch; another 25,000. If the second man's structure should fail, would it be because of the lack of a good, uniform standard of design or would it be because of his faulty conscience?

"Some years ago some men high in the profession reached the conclusion that because of the magnitude of some very large bridges these structures could be safely designed with unit stresses far higher than the small fry had to use on their small-fry structures. It is hardly necessary to remind the profession that one of these large bridges fell, and another had to have the floor system revised to make its floor area approach more nearly to its carrying capacity.

"A few months ago I had to condemn a large draw span partly because the plane of the lateral system was 4 or 5 ft. out of that of the chords, and no provision had been made for the bending stresses produced. Some of the laterals had actually bent until they were useless. It would be the simplest kind of thing to require in specifications that where lateral systems are not in the plane of the chords, provision for the bending stresses must be made in the design. This is just the kind of thing that many designers cannot see. They say it is 'their practice.' If an ironclad rule were written into specifications, it might be 'their practice' on something else, but they would learn that it is not good practice or safe design anywhere.

"The simple little requirement that the distance center to center of trusses is the span length of floor-beams, and the distance center to center of floorbeams is the span length for stringers has avoided countless disputes and has redeemed many a bridge from actual flimsiness. It is a fact that designers will, in spite of this, want to introduce their 'conscience' and use a span less than these specified distances.

"A specification writer who laid stress on professional judgment and whose work had some clauses not capable of definite interpretation, when asked personally by the writer the meaning of one of these clauses, stated that he would interpret it for a fee of $50. The clause could easily have been put in understandable English, and in such shape would be useful, for it would describe some definite kind of steel construction. All parts of specifications should be capable of only one interpretation, so that even here there is no room for judgment or so-called common sense. Ambiguity cannot help anyone, and it is apt to harm the structure.

"I have just finished inspecting some very remarkable high viaducts, running to more than 200 ft. in height. The only kind of originality they show is strict adherence to the best rules of design and workmanship. Plans were checked, material was inspected, as well as shop and field work. The steel work was carefully painted with red lead paint and has been repainted since. Conscience? Yes, every evidence of it, but conscience that has followed the rules of design and fabrication to the letter. The bridges are as good as the day they were finished. In contrast, the condemned drawspan to which reference has been made bears the stamp of a kind of professional judgment and common sense that evidently cared little for rules but thought that the structure designed was just the one for the case.

"It is not meeting the needs of the case to embody (and embalm) these rules of design in a text book or unauthoritative manual. They should be written in standard specifications where they are a law and not a mere opinion. Text books are frequently too theoretical and frequently unpractical in much that they say. There are text books that tell how to design a mill building, and there is perhaps not a mill building in existence that was designed that way. There is such a thing as being too strict and too theoretical, and there is such a thing as being too lax and leaving too much to be taken for granted. The former breeds bad structures because the strict and theoretical rules are perforce ignored; the latter has the same effect because there are no rules to apply."

In the chapter on Dams much is said concerning the grave consequences and the enormous havoc that has been wrought by the persistent notion that it is within the province of a designer to determine by his judgment whether or not the enormous force of under pressure may be neglected in designing a dam. Nature, with no regard whatever for the opinions even of high authorities, demonstrates very frequently that it is the dimensions of the structure and not the idea in the mind of its designer that gives the structure stability.

To meet the conditions of theory structures are sometimes built with far less stability than they would possess if less attention had been paid to the conditions imposed by the theory. The theory to which special reference is had is the elastic theory for arches. I have frequently advocated the designing of a concrete arch as a pin- or hinge-ended arch and the construction of the arch as fixed ended, anchoring the reinforcing rods into the abutment. Critics have said that this shows a lack of confidence in the theory. Suppose it does. A number of arches have been designed as hinged ended and great efforts have been expended to make the ends free to "hinge." The arches have collapsed. Which is better, to spend a large amount of money making freely articulating hinges for an arch and have the arch collapse or to save that money and run the reinforcing rods into the abutments for anchorage and increase enormously the factor of safety of the arch?

I do not advocate considering a reinforced concrete arch as an elastic structure, and the large amount of theoretical matter giving the involved method of figuring such arches in this way, I would wipe out of the literature on the subject. Dependence upon an intricate web of mathematical theory to sustain a structure frequently results in a type of structure which adds its meed to the inglorious failures. The reinforced concrete arch is *not* what the elastic theory assumes it to be, namely, a combination of concrete and steel both of which materials are initially unstressed. In addition to this the modulus of elasticity of concrete is a determining factor in the design of an arch by the elastic theory, and the modulus of elasticity of concrete is quite variable and uncertain.

In *Engineering News-Record*, Oct. 16, 1919, there was published an article of mine on the subject, "Common Errors in Detailing Steel Work for Buildings." This article emphasized three types of common errors in steel designing. The article and its discussion brought out a number of things of importance in the matter of safety of structures. The main features of this article will be summarized here under the headings (A), (B) and (C).

(A) Eccentric connections of beams to girders or columns frequently result in inadequate construction, though proper detailing will often remedy the fault.

A girder the plane of whose web is coincident with the center line of a column, if it has a good substantial riveted web connection to the column is in effect not an eccentric load on a column, though the same girder, if resting on a bracket on a column, may produce excessive bending in the column. The riveted web connection in effect continues the girder to the center of the column, and the girder itself will take

any bending moment that may exist at the column. Exception was taken to this in the discussion which followed, but I maintain that it is sound engineering and an application of the true theory that recognizes toughness as well as elasticity in steel. By the theory of perfect elasticity the case of the girder which is rigidly attached to the column may show a greater bending moment in the column than the case where the girder rests freely on a bracket. But if there is excessive bending (theoretically computed) in both cases, the girder would effectively prevent failure in the first case, whereas in the second case, the girder would have no influence whatever in preventing the bending and failure of the column. This is the feature of sound engineering that my critics missed altogether.

My paper recommended the balancing of loads on columns, the use of top and bottom flange connections of beams to columns to throw bending moments into beams instead of allowing them to be taken by columns by eccentric bracket connections, the extending of beams and channels past a column to act as supports for other beams that would otherwise be on eccentric bracket connections, the turning of columns so that bending moments will be resisted in the strong way of the column, the use of knee braces to stiffen up light columns subject to eccentric loads, and finally provision in all columns for the bending stresses by increase of section where unavoidable bending moments occur.

(B) In detailing, cantilever beams are frequently inadequately provided for in the matter of top and bottom flange stress. The standard end connection suitable for a simple beam does not answer the purpose where a steel beam is compelled to carry a cantilever load. A standard end connection in connection with a top flange plate connecting the cantilever beam with another beam on the same line may be sufficient, or the case may require both top and bottom flange plates.

(C) Beams that are curved in plan should either have intermediate supports or be provided with end supports similar to those required for cantilever beams. Also the flanges of such beams should be well braced since the tendency is for the stress to straighten out the tension flange and put more curve in the compression flange.

In the discussion which followed the publication of my paper a number of engineers took exception to my stand that theory rightly interpreted and practical considerations, as well as proper stability, would dictate a rigid connection between a girder and a column and the consideration of such construction as though the girder were centrally supported by the column. Mr. T. S. Needels added a constructive suggestion, namely, that where beams frame in near the tops of girders

they should be extended across the top rather than be placed on a bracket reaching beyond the girder flange, this to avoid twisting on the girder.

In the paper above referred to I used the terms "practical designer" and "ultra-theorist," as I have frequently used these words. I feel that my definition of these terms is in place in this chapter, hence the following quotation from *Engineering News-Record,* Jan. 22, 1920.

"By practical designer I do not mean the man who ignores theory and designs by guess and the rule of thumb. I refer simply to the class of men who are doing the practical designing, men who make it their business to design. In this class there are a few men who are up in all the intricacies of theory, but the average designer, while he understands the basic theories of design, can follow the theorist so far, and then he loses the way. Rules of design for him must be predigested. If finespun theory is encountered in the way to the solution of a problem, he sidesteps it; and very frequently in his design, he simply ignores things that are of vital importance, because he sees the theoretical lion in the way. For this reason there is often a great hiatus in the work of the practical designer. It is to help this class to do better work that I am striving to induce the engineering profession to simplify their theory so as to eliminate superfluous matter, the presence or absence of which has no bearing whatever on the safety or economy of structures, unless it be that it adds greatly to engineering cost in the design.

"The ultratheorist is the man who plays on an instrument with one theoretical string, and that is the perfect elasticity of steel. If the ultratheorist were inclined to take all facts and theories into his problem and weigh their relative merits, he would perhaps discard some of his intricate theories; for the solution of some problems, if every fact were considered is so stupendous that the thing would fall from sheer weight. As an example of an ultratheorist, a certain author takes up pages of his book, in a most complex theory of earth pressures, to work out a retaining-wall formula on an assumption of properties that no earth on this planet possesses.

"Horse sense is theory in oilskins and rubber boots. It is as much theory as the elasticity of steel. But it is the fag ends of theory that do not admit of nice mathematical solution. It is just as much a scientific fact that steel is tough as that steel is elastic. But the toughness of steel, while it saves the life of countless structures and men, is erratic and not subject to nice laws, such as an assumption of perfect elasticity. Hence, the ultratheorist prefers to work out his problems on the perfectly elastic theory.

Reprint from *Bulletin of the Society for the Promotion of Engineering Education,* April, 1919.

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Instruction in the details of structural design, from results that crop out in practice, in the experience and observation of the present writer, in his judgment, is in need of radical realignment. A number of diverse sets of facts that are the basis of the statement in the foregoing sentence will be given.

Some years ago I examined the detail drawings of two theaters which had been made by an engineering concern. The errors in details were so numerous that I did not count them. I classified them instead. There were twenty-two different kinds. Some of them were repeated many times. A number of them were so serious that they might have resulted in collapse of parts of the structure. I wrote these up for the benefit of the profession, and the article was published in *Engineering and Contracting,* December 8, 1915. Previous to this time I had written up a description of a large collection of errors in structural details from different plans that I had examined. (See *Engineering News,* April 11, 1907.) Some of these errors were similar in character to the others, showing the same disregard for fundamental principles of stability.

Now, it is of no particular significance that an individual, though holding himself out to be a structural engineer, should make blunders and errors. It is significant, however, that this engineer was unable to see that there was anything wrong with his details, though the builder, a contractor with no engineering training except the horse sense that his experience as a contractor gave him, could see all of my points and insisted, even at much expense to himself, upon everything being made as I dictated.

The reader is referred to the published articles to see for himself whether or not the details were faulty.

But the most significant thing of all is that I was in a position where I could refer to no authority to sustain my points and to no writing except my own.

In engineering works and in the engineering class a structure seems to be a highly theoretical thing made of perfectly elastic substances that are subject to certain inexorable laws and obey those laws implicitly. But standing supreme over all of these laws is the professional judgment of the designing engineer, which ultimately sustains the loads. In my opinion the professional judgment of the designing engineer is worth practically nothing, unless he can amply sustain it

by arguments that will stand the light of day and show that he has plain horse sense as well as some theoretical knowledge. In my opinion the theory of least work, and of redundant stress, and of the elastic behavior of concrete arches, and of secondary stresses due to such causes as the rotation of members on pins due to truss deflection, and most column theories, and several other nice theories, have a value only a little greater than that already ascribed to bald professional judgment.

To come right to the point, there is entirely too much theory, and there are entirely too many theories in engineering works and college teaching and not enough ordinary horse sense.

I could write a book on this subject, showing where engineers, among them some of the highest standing, have designed and discussed structures with their heads away up in the rarefied air of pure theory, while the details, that a man with scarcely any theoretical training could see were faulty, never came into their view nor concern. A few of the high spots will be touched.

When the Quebec bridge was first erected, it was put up with a great traveler as large as a 16-story steel office building. It consisted of two side bents 217 feet high. Now horse sense tells us (though books and theory say little) that two bents of this height ought to be braced together to prevent their swaying to one side and flopping down, especially if one million pounds of weight rests on top of the bents. Did the engineers provide this bracing? No. Well, what happened? Why the two bents did tilt over, with their million-pound burden, and pulled the completed bridge over, killing about 80 men. Did the Royal Commission investigating the wreck give due warning to the profession against the recurrence of another such blunder? *In eight pounds of dope issued by the said Royal Commission there is not a word about any such possibility.* There are some nice new theories, but so far as a discussion of the real cause of the failure is concerned this report is one of the greatest farces in engineering literature.

But the Quebec bridge was rebuilt. A well-braced traveler was employed. I had publicly called attention to the blunder of the first and prophesied that in the rebuilding no such blunder would be made. Again the designing engineers had their heads in the rarefied air of pure theory and high-class problems—a la the most approved literature of engineering. Great problems were solved. The solutions do credit to the designers and to the men who handled and erected the steel. Again a detail was overlooked—a detail that it required horse sense to discover. A great load was to be lifted on a swinging support. The load was to be suspended from a long line of bars and jacked up to place. The lifting scheme was excellently worked out. But why was there a second wreck?

Makers of pails for paint suspend the pail on a bail attached as close to the top as the bail can conveniently be attached. These men know, without being able, perhaps, to tell the reason why, that if the bail is swung, say a half inch above the middle of the height of the pail, you may be able to lift the pail without its upsetting, but the chances are that it will upset and spill the paint. Now, in raising the great suspended span of the Quebec Bridge the "bail" was hinged the trifling distance of 2¾ inches above the "center of the pail." The pail upset, after it had been lifted a few feet. Did the profession see this and publish the lesson broadcast so as to warn others? Look for yourself at current engineering publications for the few weeks following the disaster and see if you can find it. I did succeed in getting a letter published which pointed out the real cause of the wreck. (See *Engineering and Contracting,* October 25, 1916.)

The Orpheum Theater in New York failed completely and solely because of a monstrous curved girder of 74 feet of a span and 12 feet of an offset or versedsine, and this girder was carrying a heavy floor load. The only thing that supported this girder till its collapse was small beams that it was supposed to support. The girder flopped down and pulled with it the whole structure, just a few minutes after 200 workmen had left the building. A large and important part of engineering education is in the hands of engineering periodicals. Was the engineering profession "educated" in the accounts of this wreck? Hear the history. After about a year of investigation and delay a set of the most absurd and inane reports were published that I have ever had to read. No mention was made of the real cause of the wreck. I succeeded in getting in a statement of the cause in *Engineering and Contracting,* February 18, 1914.

What do the authorities say on the instability of curved girders? Find it, if you can. You will find some nice theories on circular girders and some applications of the same where they are totally inappropriate. Find a warning, if you can, against using a curved girder with simply supported ends. But in current engineering publications after this wreck here is what you will find—some fine spun theory on the dire effect of the twisting of a girder due to deflection of a cross girder that it supports. Bosh!

Four hollow dams have failed because the pressure on the wetted side and the pressure of water beneath them was not adequately resisted by the small weight of concrete in the hollow dam. What do books on dams say of under pressure? If the word of these books is to be depended upon for information on this subject, under pressure did not exist until 1910, or, by majority vote, it does not exist at all. One book, published since 1910, emphasizes the existence of under pressure.

Others do not even give it the recognition of mention. I believe that instructors have at last begun to emphasize under pressure. They ought to refuse to teach from any book that does not emphasize the necessity of designing for under pressure.

When the Stony Creek Dam, a hollow dam that failed, was rebuilt, the description of the work stated that the type of dam was in no way responsible for the failure. Yet in the rebuilding the type was changed in these important respects: it was securely anchored to a deep cut-off wall; a lot of concrete was added at the up-stream edge to give stability; a very extensive drainage system was added; and, most radical of all, the drainage system was housed in at great expense to keep it from freezing. When I, a member, attempted to discuss the matter and point out these facts in the *Proceedings of the American Society of Civil Engineers*, I was denied the privilege. This is a sample of the way engineers are being "educated."

There is enough written on the theory of the elastic arch as applied to reinforced concrete arches to fill several books. I have looked in vain for some recognition of one of the biggest single things in this design of an elastic reinforced concrete arch, namely the bending moment at the spring of the arch and provision for taking care of it. For the sake of saving a little concrete in the arch ring a highly theoretical method of design is employed, and the abutment is assumed to be absolutely fixed whether its mass is sufficient to warrant this assumption or not. This method of design is based on pure theory. The elastic theory stands or falls on this premise, namely, whether or not the deflection of a reinforced concrete beam can be calculated. The deflection of a simple reinforced concrete beam cannot even be closely approximated; hence the elastic theory has no real foundation to stand on.

The use of the simple equilibrium polygon in solving a reinforced concrete arch is so simple and so appropriate to the case that it is a puzzle to me why the elastic theory should ever be brought to bear on the problem.

In reinforced concrete theories are rampant. The rodded column is based, in its use, on theory almost solely. The rodded column is a shaft of plain concrete with a rod near each corner, the rods being stayed at intervals of a foot or so by wires. Sometimes there are more than four rods. Sometimes they are bad enough to have rods in the middle of the side of a rectangle "stayed" with a wire in the same plane. The only thing supporting the use of a rodded column besides pure theory is a lot of misleading tests—tests of columns perfectly centered in an almost perfect testing machine. This is worse than theory because it inspires more confidence. A column, to be a proper member of a structure,

must be tough. The rodded column is anything but tough, but the testing machine does not discover this.

In a score or more of bad wrecks rodded columns have proven their utter unfitness as structural members by their failure to sustain their loads and by their behavior in the wrecks. Apart from this I know of only one test of reinforced concrete columns where the thing tested resembled an actual structure and really tested a rodded column in the way it is tested as a building column. This test is described in *Engineering Record,* September 30, 1905. Here four *rodded columns* carrying two girders and a slab *failed at about 400 lbs. per sq. in.*

The theory of the rodded column does not consider the deflection of the beams and girders connecting in a monolithic character to the columns, and the unknown eccentric stresses at the corners, and the tendency to spall on account of this eccentric stress and the shrinking of the concrete that compresses the steel and aids in this spalling. On the other hand the same class of theorists who thus ignore facts concerning concrete, a very brittle substance, will split the hair of a gnat in making up theories of secondary stresses in the heavy members of a steel structure. A steel column may have a tremendous unit stress at one corner, due to poor milling, and nothing may result except the peening of the metal. A rodded column having a corresponding condition, due to deflection of the beams, will be sure to spall and may fail and bring down the structure with it. These things are theory in one sense, but they are theory that recognizes facts and is not blind in spots. These facts are eminently common sense, and therefore it is hard to make an ingrained theoretical mind see them. Hence we have the common standards of design in reinforced concrete that are prepared and approved by two classes of men: one of these classes will accept anything that has some authoritative approval, so that it will enable them to compete with other forms of construction; the other class are laboratory investigators who cannot see any difference between an ideal laboratory experiment and a building and who make no attempt to simulate the laboratory experiment to the building.

The stirrup or short shear member in a reinforced concrete beam is another theoretical creation. Someone with a theoretical knowledge of a truss conceived the idea that stirrups or short shear members, so called, since they look like truss members on paper, can be considered as truss members. The resemblance does not extend beyond the drawing board, for truss members must have perfectly definite and full-strength connections to both top and bottom chords. Stirrups and short shear members have no such connection either to top or bottom chord or flange of a beam.

No engineer will attempt to explain how a stirrup acts to take its alleged stress nor to explain what takes the shear of a beam between two stirrups or short shear members. Engineers have ceased to discuss both rodded columns and stirrups. They are ashamed of the alleged arguments in favor of these discredited features of design. As proof of this I point to a discussion on these subjects that took place in February, 1915, when I was severely castigated in a meeting of the American Concrete Institute in Chicago. After much insistence on my part the remarks of my critics were written out (most of them) and I answered them. But the men who made the remarks were ashamed of the weakness of their arguments and had sufficient influence to prevent the publication of the discussion. I have been assured repeatedly in the last four years that the discussion would be published, though I have been convinced in my own mind from the start that these men would never let it be published.

I have a standing challenge with the engineering profession to discuss both rodded columns and stirrups and publicly declare the standards of design as regards both, and dependence upon the same, to be outrageous. But the engineer who will dare to risk his reputation by standing up publicly and alone for these absurdities has not yet arisen. In bunches and committees they will pass reports and frame codes that incorporate these false standards of design, but individuals fight shy of being held responsible for something that will not stand the light of common sense.

The reinforced concrete flat slab has given rise to some complex theories that are of no more use than discussions as to how many angles can stand on the point of a needle. The flat slab between two rows of columns is no better than a similar slab between two rows of beams and needs no more complex theories to discover its limitations, interested mathematical prestidigiteurs to the contrary notwithstanding. This simple criterion is the one that should govern both in the making of a formula for design and in tests. But the men who have made the formulas and the tests have ignored this.

About the only practical test on a flat slab building that was a critical test was that made on the Bell St. Warehouse. (See *Engineering News*, January 29, 1916.) This test failed. Investigators have tested interior spots but they fight shy of a row of bays on the outside of a building where no girder exists or a row of bays across a building from outside to outside.

The flat slab and the rodded column, where tested critically, have failed to meet the requirements of the design by which they were constructed.

Now the charge I wish to make against the educators of the structural engineering profession is that by failing either to defend or condemn these wreck-breeders they are failing in their duty. Too much time is given to theoretical discussions that lead nowhere and create false impression of security, and too little time is given to common-sense discussion of the obvious causes of all wrecks, which need no higher mathematics to understand.

The engineering profession is living in a fool's paradise of fine-spun theory, and when a wreck occurs from some simple cause, they spin more fine theory to cover up the real cause of the wreck.

What ought to be done is this: cut out a lot of fine theory or let it lie embalmed in the books where it was born, and give a course in common horse-sense in the details of structural design. Let editors open their columns to a free discussion of the indefensible, the bad, the wreck-breeding features of design in reinforced concrete and steel structures and in dams.

I have written this at the request of the chairman of the Committee on Methods and Details of Teaching Structural Design. It is not written to advertise my books or writings. If there was any place else to which I could refer for material for a course such as I suggest I would gladly mention it. I have learned that the way to write books that will not sell is to say in them the things that others have not said and perhaps will not agree with. Any instructor who wishes material for a lecture course in horse sense in structural designing may be able to find these writings in some large library.

L'ENVOI

As this book goes to press I have just had returned to me a contribution to a discussion on a purely theoretical paper, on baseless assumptions, which the editor has slashed to the extent that scarcely three lines are untouched. My meaning has been softened, and obscured and reversed; but the reason is not "established fact controverted," it is the price of printing and paper. At least three papers of this character have used up a large fraction of a thousand pages of the society's journal in a year.

SOME LETTERS, ARTICLES, PAPERS, CONTRIBUTIONS TO DISCUSSIONS AND BOOKS ON ENGINEERING AND SCIENTIFIC SUBJECTS, WRITTEN CHIEFLY IN AN EFFORT TO ADVANCE SAFE DESIGN AND SIMPLIFIED METHODS OF DESIGN

BY EDWARD GODFREY.

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